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THE TORONTO MEETING OF THE BRITISH ASSOCIATION.

THE British Association decided to meet in Montreal in 1884, with some hesitation, but after the great success of that meeting an invitation presented at Oxford in 1894, urging the Association to meet at Toronto in 1897, was accepted by the general committee with unanimity. Elaborate arrangements were made by the Dominion, by the City of Toronto and by the University, for the reception of the Association, and steamship, railway and cable companies united to offer unusual facilities to visitors. The chairman of the local committee, Professor Macallum, was especially self sacrificing in devoting himself for more than a year to the work of arrangement. A visit of the leaders of British science to Canada is an event of no common importance for the Dominion, and the enthusiasm was this year intensified by a United States tariff law and a British Jubilee.

The members and associates in attendance at Toronto numbered somewhat over 1,300, about 400 fewer than at the Montreal meeting. SCIENCE has already published an article by Professor Macallum giving a list of the more distinguished British and Continental men of science who expected to attend, and with a few exceptions, such as Lord Rayleigh, they were present. Men of science from the United States were well represented. The attendance from Great Britain, the Continent, Canada and the United States can best be indicated by giving the officers and committees of the various sections, which were as follows:

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Nearly all these scientific leaders and many others were present at Toronto when the Association was received by the civic authorities, at four o'clock on the afternoon of Wednesday, August 18th. Addresses of welcome were made by Lord Aberdeen, the Governor-General, and by the Mayor of the city. Replies were made by the retiring President of the Association, Lord Lister, by the incoming President, Sir John Evans and by Lord Kelvin. The Premier of the Province and the Minister of Education also made remarks. In the evening Lord Lister resigned the presidency to Sir John Evans, who gave the address already published in SCIENCE (page 269). Remarks thanking the President for his address were made by Lord Aberdeen, Lord Kelvin and Mayor Shaw.

Other general meetings of the Association were held to listen to the evening addresses by Professor W. C. Roberts-Austen and by Professor J. Milne. Professor Austen, on August 20th, lectured on 'Canada's Metals'. He reviewed the discovery and prospects of Canadian gold fields, illustrating the subject by a series of photographs, and called special attention to the nickel deposits of the Dominion. Illustrations were also shown of the speaker's interesting experiments on the splash of metals, and a demonstration was given showing on the screen the action of the electric furnace. Professor Milne, on Monday evening, lectured on a subject in which he is the leading authority, 'Earthquakes and Vol-

canoes.' He discussed especially the causes leading to the display of seismic and volcanic activity and the benefits to be derived from studying the phenomena. Dr. H. O. Forbes gave, on Saturday evening, the usual lecture to artisans, taking as his subject 'Borneo.' Meetings were also held for the conferring of degrees on several of the prominent members both by Toronto University and by Trinity College.

The arrangements for social intercourse and entertainment were numerous and attractive. A reception was given by the Governor-General and the Countess of Aberdeen on Thursday evening; there was a conversatzione in the University building on Tuesday evening and a banquet in honor of Lord Kelvin, Lord Lister and Sir John Evans on Wednesday evening. There were garden parties daily in the afternoons and many luncheons and dinners. Excursions were made on Saturday to Niagara Falls and in other directions, and for the Thursday following the meeting excursions were arranged to Montreal, Ottawa and elsewhere. Finally elaborate excursions were planned under the most favorable conditions to the maritime provinces and to Manitoba and the Pacific coast.

The business of the Association is transacted by the General Committee, chiefly on advice of the Council. This year part of the arrangements, including the election of officers for next year, were deferred until an autumn meeting in Great Britain. Professor Roberts-Austen was made General Secretary in succession to Mr. Vernon Harcourt, and in the room of the five members of the Council who retired there were elected F. Darwin, Esq., Sir C. W. Freemantle, Professor W. D. Halliburton, Professor S. P. Thompson and Sir W. H. White. Dr. F. Kohlrausch, Berlin; Dr. Van Rejckevorsel, Rotterdam, and Professor E. Zacharias, Hamburg, were made corresponding members.

The report of the Council adopted by the General Committee contained reports of two important committees appointed at the Liverpool meeting. The committee appointed to bring before the government the question of the establishment of a national physical laboratory has been in so far successful that the Treasury is taking the matter into consideration and has appointed a committee to report on such a laboratory.

The committee appointed to urge upon the government the importance of establishing a Bureau of Ethnology for Greater Britain, consisting of the President and General Officers, with Sir John Evans, Sir John Lubbock, Mr. C. H. Read and Professor Tylor, made the following report:

A central establishment in England, to which would come information with regard to the habits, beliefs and methods of government of the primitive peoples now existing would be of great service to science and of no inconsiderable utility to the government.

1. The efforts of the various societies which have, during the last twenty years, devoted themselves to collecting and publishing ethnological information have necessarily produced somewhat unequal, and therefore unsatisfactory, results. Such societies had, of course, to depend upon the reports of explorers, who usually travelled for another purpose than that in which the societies were interested; and such reports were naturally unsystematic, the observers being mostly untrained in the science. Again, whole regions would be unrepresented in the transactions of the societies, perhaps from the absence of the usual attractions of travellers, *e.g.*, big game or mineral riches. This has been to some extent corrected, at least as to the systematic nature of the reports, by the publication of 'Anthropological Notes and Queries,' by the Anthropological Institute, with the help of the British Association.

If it be admitted that the study of the human race is an important branch of science, no further argument is needed to commend the gathering of facts with regard to the conditions under which aboriginal races now live, and if this work is worth doing, it should be done without delay. With the exception, perhaps, of the negro, it would seem that none of the lower races are capable of living side by side with whites. The usual result of such contact is demoralization, physical decline and steady diminution of numbers;

in the case of the Tasmanians, entire disappearance. Such will probably soon be the fate of the Maories, the Andamanese, the North American Indians and the blacks of Australia. While these exist it is possible to preserve their traditions and folk-lore and to record their habits of life, their arts and the like, and such direct evidence is necessarily more valuable than accounts filtered through the recollection of the most intelligent white man.

It is scarcely necessary to enlarge upon this point, as no one will seriously question the value to science of such information. But it does seem necessary to urge that no time be lost.

2. As to the benefit, to the government, of these inquiries, the history of our relations with native tribes in India and the colonies is rich in examples. No one who has read of the ways of the African can doubt that a thorough study of his character, his beliefs and superstitions is a necessity for those who have to deal with him. And what is true of the natives of Africa is also true, in a greater or less degree, of all uncivilized races. Their ideas of common things and common acts are so radically different from those of civilized man that it is impossible for him to understand them without a special training.

Even in dealing with the highly civilized natives of India it is most necessary that an inquirer should be familiar with their religion and with the racial prejudices which the natives of India possess in common with other civilized nations.

A training in knowledge of native habits is now gone through by our officers, traders and missionaries on the spot; and by experience—sometimes dearly bought—they, after many failures, learn how to deal with the natives. By the establishment of such a bureau as is here advocated much might be done to train our officers before they go out, as is now done by the Dutch government, who have a handbook and a regular course of instruction as to life, laws, religion, etc., of the inhabitants of the Dutch Indies. The experience thus gained would then mature rapidly, and they would become valuable servants to the state more quickly.

The collecting of the necessary information for the Bureau could be done with but little expense and with a very small staff only, if the scheme were recognized and forwarded by the government. If instructions were issued, for instance, by the Colonial Office, the Foreign Office, the Admiralty, and the Intelligence Branch of the War Office, to the officers acting under each of these departments, not only that they were at liberty to conduct these inquiries, but that credit would be given to them officially for good work in this direction, there is little doubt that many observers qualified by their previous training would at once put themselves and their leisure at the disposal of the Bureau.

The Bureau itself, the central office, would be of necessity in London—in no other place could it properly serve its purpose—and preferably for the sake of economy and official control, it should be under the administration of some existing government office. But the various interests involved make it somewhat difficult to recommend where it should be placed. The Colonial Office would obviously present some advantages. The British Museum has been suggested, with good reason, and there appears to be no insuperable difficulty if the Trustees are willing to undertake the responsibility of controlling such a department.

The staff would not be numerous. A Director accustomed to deal with ethnological matter would necessarily direct the conduct of the inquiries, and until the material assumed large proportions two or three clerks would probably suffice. If the value of the results were considered to justify it, the increase of the area of operations over the world would probably call for additional assistance after the Bureau had been at work for a few years.

The Bureau of Ethnology in the United States aims chiefly at publishing its reports, but its area is limited to America. The scope of the present proposal is so much wider that the Committee think it better not to deal with the question of publication at present.

If this report be adopted by the Council it will be necessary to approach the government and impress upon them the importance of having such an organization for carrying out these recommendations. For this purpose a deputation should be appointed, and it would be well to invite the Council of the Anthropological Institute to appoint two members."

The Council resolved that the Trustees of the British Museum be requested to consider whether they could allow the proposed Bureau to be established in connection with the Museum; and if they are unable to sanction this proposal, that the authorities of the Imperial Institute be requested to undertake its establishment. The matter is now under the consideration of the Trustees of the British Museum.

The Report of the Corresponding Societies Committee for the past year, together with the list of the corresponding societies and the titles of the more important papers, and especially those referring to local

scientific investigations, published by those societies during the year ending June 1, 1897, was received.

One of the most important functions of the Association is represented by the work of its committees. In addition to the three reports to the Council many very valuable reports were presented at Toronto before the sections, which will be noticed in special articles on the work of the sections to be published in subsequent issues of SCIENCE.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

*LONG RANGE TEMPERATURE AND PRESSURE VARIABLES IN PHYSICS.**

METHODS OF PYROMETRY.

THE endeavor to provide suitable apparatus for high temperature measurement is one of long standing. The student of the subject is fairly overwhelmed with the variety of devices which have been proposed. There are few phenomena in physics which have not in some way or other been impressed into pyrometric service, often indeed by methods of exquisite physical torture. I cannot, of course, even advert to many of these this afternoon, as my purpose will have to be restricted to such devices as have usefully survived. Thus a whole group of 'intrinsic thermoscopes,' as Lord Kelvin calls them — apparatus in which some property of the substance is singled out for measurement—will be overlooked. Pyrometry will some day receive substantial aid from the phenomena of solid thermal expansion, dear to the hearts of old Wedgewood, of Professor Daniells, of the citoyen Guyton-Morveau, and recently to Professors Nichols, Joly and others; but even the 'meldometer,' which has received Ramsay's encouragement and recent heroic attempts to measure the expansion of platinum, have not yet entered the arena to stay.* The same may be said of vapor pressure, ebullition and certain dissociations, of which the former is entirely too liberal in dispensing pressure, and the latter too negligent in readjusting it. Little has been done with heat conduction regarded as subservient to the measurement of high temperatures; little with color and the spectrum, even though Draper and Langley in this country and many others abroad have paid tribute; little with polarization. The wave-length of sound has told Cagniard Latour and our own A. M. Mayer much about high temperature, but it did not tell them enough.

Throughout the history of pyrometry, *fusion* seems to have come forward for journeyman duty. What is more convenient than to find whether the degree of red heat is too low or too high from the fusion of prepared alloys. As far back as 1828 Prinsep, aware of the golden opportunity, with his golden air thermometer determined the melting point of some equally precious alloys of gold, silver and platinum, and determined them very well. Other alloys were afterwards substituted and graded mixtures made of quartz, chalk, kaolin and feldspar for the purpose. Efforts to obtain more accurate values are due to Becquerel; but the absolute values most widely used until quite recently, namely, the melting points of silver (958°), gold (1035°), copper (1054°), palladium (1500°), platinum (1775°), iridium (1950°), are due to the researches of Violle.

Interest in high temperature fusions has of recent date rather increased than abated. The demand for more accurate data has been met by the Reichsanstalt, and we have now a set of values for silver, copper, gold, nickel, palladium and platinum in terms of

* Address by Professor Carl Barus, Vice-President and Chairman of Section B (Physics).

* Noteworthy attempts to replace mercury by a liquid potassio-sodium alloy in glass thermometers are among the novelties.

the air thermometer standard of that institution. Data have also been supplied by Callendar. Among these values there is as yet considerable confusion and the end is not yet. Long ago I suspected that the Violle melting points were probably too low, whereas the assumed zinc boiling point is probably too high. This surmise has been partially borne out by the Reichsanstalt, though Le Chatelier even now prefers Violle's values. *

Thermoscopes based on a *specific heat* have an advantage over fusion thermoscopes in not being discontinuous. They are quite as 'intrinsic' and much less convenient in practice. Guyton-Morveau, at the beginning of the century, pointed out the pyrometer importance of specific heat, and a host of observers followed him. But the critical discussion of the subject is due to Pouillet (1836), who determined the thermal capacity of platinum between 0 and 1200° absolutely, and found a value so nearly constant as to place this method of pyrometry in a very favorable light. Other observers followed with new data, and the bulk of our knowledge to-day is again due to Violle. Violle used Deville and Troost's exhaustion air thermometer and determined the law of variations of specific heat and temperature throughout a large pyrometric interval, with a number of metals, silver, gold, copper, palladium, platinum, iridium, among them. It was by prolonging this law as far as fusion that the melting points of the metals, to which I have already alluded, were obtained. This verges on extrapolation, but it is not extrapolation gone mad.

The importance of calometric high temperature measurement has recently been accentuated in connection with the remarkable high temperature accomplishments of Moissan. Furnace temperatures in the case of such technological operations as are used in connection with iron, glass and porcelain manufacture rarely exceed 1,400°, except perhaps in the Bessemer process, where the temperatures are wont to exceed 1,600° and even reach 2,000°. In Moissan's furnace, which is essentially an electric arc enclosed by non-conducting lime, a totally new order of high temperatures is impressed. There was thus a call for at least an approximate measurement of their values, which was answered by Violle, assuming that the specific heat of carbon above 1,000° approaches a limit. The sufficiency of this hypothesis is not unchallenged, however; for instance, Le Chatelier finds that, up to 1,000°, the specific heat of carbon continually increases, having no certain limit. Admitting Violle's results, Moissan's furnace temperatures exceed 2,000° even at 30 amperes 55 volts; at 360 amperes and 70 volts tin and zinc oxides melt and boil; they exceed 3,000° at 500 amperes and 70 volts, where lime melts, and often boils. Moissan, however, went as far as 1,000 amperes at 50 volts.

The striking novelty of Moissan's work is rather of chemical interest, and a large part of it is so fresh in our memory that, in view of Moissan's forthcoming book,* I need merely glance at it. A range of fusibilities, among which platinum lies lowest, while chromium, molybdenum, uranium, tungsten, vanadium, follow in order, and of ebullitions beginning with silica and zinc oxide, is rather breath-taking. Finally, his structural investigations on the occurrence of carbon, and his long series of carbides, many of them commercially valuable, have staggered even the sensational press.

Leaving other intrinsic thermoscopes for the moment, I will ask your attention in

* The following table contains a brief summary:

Ag.	(Violle) 954°	(Barus) 986°–985°	(Callendar) 982°	(H.&W.) 971°
Au.	1045°	1091°–1093°	1037°	1072°
Cu.	1054°	1096°–1097°		1082°
Ni.	—	1476°–1517°		1484°
Pd.	1500°	1585°–1643°		1587°
Pt.	1775°	1757°–1855°		1780°

* Le four électrique, par Henri Moissan; Paris, Steinheil.

this place to the development of the only fruitful method of absolute pyrometry which has yet been devised. I refer, of course, to the gas thermometer, or, in other words, to the manometric methods of measuring the thermal expansion of gases. Efforts have, indeed, been made to use gaseous viscosity for absolute high temperature work. It has been definitely pointed out, inasmuch as viscosity in gases is independent of pressure, while both viscosity and volume increase with temperature, that the transpiration rates of gases through capillary tubes of platinum glazed externally would necessarily be an exceedingly sensitive criterion of the variation of high temperatures. The small volume of the transpiration pyrometer as compared with the clumsy fragile bulb and appurtenances of the air thermometer is further to the point. But modern kinetics has as yet failed to fathom the law of variation of viscosity with temperature, and even the researches of O. E. Meyer in this direction do not seem to have quite touched bottom. Gaseous transpiration pyrometry is thus still much in the air, surveying the horizon of a glorious future.

Returning from this digression to the air thermometer, we find the first thoroughgoing piece of high temperature work carried out by Prinsep (1829), by the aid of a reservoir of pure gold to which I have already alluded. Prinsep's manometer was filled with olive oil, and the volume issuing at constant pressures was found by the balance. In view of the pure olive oil, probably available in 1829, these experiments must have been comfortably appetizing, and I dare say Prinsep's good humor in the matter may have contributed to the remarkable excellence of his results. Prinsep's researches were not superseded until Pouillet, in 1836, published his paper on pyrometry. Pouillet constructed an air thermometer bulb of platinum and was thus able to reach the farthest pyrometric north of the day and long after. His results are many sided; they contain the first definite data in radiation pyrometry and in calometric pyrometry. His constant pressure manometer, afterwards further perfected by Regnault, is the best apparatus for the purpose to-day. Pouillet did not suspect, indeed he remained quite unaware of, the permeability of platinum to furnace gases; perhaps for this and other reasons he failed to detect the thermo-electric anomalies in the platinum-iron couple which he so carefully calibrated.

It was thus a great step in advance when Deville and Troost long after replaced platinum by glazed porcelain, availing themselves (1857–60) of Dumas' famous vapor density method for measuring temperature. Unfortunately for the rapid progress of pyrometry, Deville and Troost used iodine vapor in their bulbs, a heavy gas indeed, but a gas, as was afterwards found, whose low temperature molecule dissociates at higher temperatures. Thus they unwittingly committed an even greater error than Pouillet in gliding over permeable platinum, and their data for the boiling points of zinc and of cadmium were about 100° too high. In fact, these results were challenged not long after by Becquerel (1863), who had fallen heir to Pouillet's platinum air thermometer, had used it to calibrate a platinum-palladium thermocouple of his own, and had found data for the boiling points of zinc and cadmium upwards of 110° below those of Deville and Troost. I cannot here enter upon the discussion which thereafter arose between these active observers further than to state that in the course of it both parties frequently repeated their measurements (Becquerel even substituting a porcelain bulb for Pouillet's thermometer) without removing the decrepancy between their values.

Later researches have decided in favor

of Becquerel's results, and his original research, with its applications to fusion, to radiation, to thermo-electrics, etc., is one of the noteworthy accomplishments in the history of pyrometry. Nevertheless, it must not be forgotten that to the investigation of Deville and Troost our knowledge of the perviousness of iron, platinum and other metals to gases is due. We are also indebted to Deville for the great discovery of dissociation, the very phenomenon which he was here so loth to acknowledge. This is the case of a man stumbling in his own footprints. Victor Meyer was, I believe, the first to point out the probable dissociability of the iodine molecule, suggesting a fruitful subject of research which has since been extended to many other molecules.

In 1863 Deville and Troost began a new series of high temperature researches, the feature of which is the perfected form of porcelain bulb. This was a hollow sphere and long capillary stem adapted for use with Regnault's standard air thermometer. Great difficulties were encountered in the endeavor to glaze the bulbs within. They were finally overcome by making bulb and stem separately and then soldering them together with feldspar before the oxy-hydrogen blow pipe. Elaborate measurements on the thermal expansion of Bayeux porcelain accompany these researches which, undertaken together with M. Gosse of the Bayeux works, occupied them intermittently for about seven years. A full summary of their data did not appear, however, until 1880, when, together with a new vacuum method of high temperature air thermometry, they communicated the results of twenty-seven measurements on the boiling point of zinc. Their new results are in good accord with the data of Becquerel already cited and the more recent results of Violle and others for the same landmark in the region of high temperatures. Measurements between 0° and 1500° had thus reached a degree of precision of about 15° in 1000°.

The further development of pyrometry took a somewhat different direction. Regnault (1861) had already made use of a displacement method, in which the measuring gas is removed bodily into the measuring apparatus by an absorbable gas. But the method was independently revived by Professor Crafts, of the Boston Institute of Technology. These methods are not of especial excellence below 1500° ; but above that temperature, when most solids tend to become viscous, their importance increases (as Crafts duly pointed out) in proportion to the rapidity with which the measuring operations can be completed. One or two minutes may suffice and different gases may be tested consecutively. It is in this way that Victor Meyer and his pupils, after demonstrating the dissociation of iodine and chlorine molecules, succeeded in penetrating quantitatively to very much less accessible heights of temperature. A particular desideratum was a rigid test as to the stability of the molecule of the standard measuring gases (oxygen, hydrogen, nitrogen). The results were favorable, inasmuch as for these and for many gases like CO_2, SO_2, HCl, Hg, etc., the expansions obtained were linear functions of each other.

In their final work, temperatures as high as 1700° were reached, the air thermometer for this purpose being tubular in form, consisting of very refractory fire clay with an interior and exterior lining of platinum and with two end tubulures of platinum for influx and efflux of gases. Among many results of great chemical interest their researches showed that metallic vapors, phosphorus, sulphur, etc., at high temperatures tend to pass into the monatomic or the diatomic molecular structure.

Sometime after (1887) a series of experiments furthering the line of research of

Deville and Troost were made, with a geologic aim in view, in the laboratory of the U. S. Geological Survey. Finally, porcelain air thermometry was taken up with great vigor by the Reichsanstalt. These results, due to Holborn and Wien, are now almost exclusively quoted and carry the stamp of the great institution from which they emanated. They have been wisely made commercially available by the deposition, with Heräus, in Hanau (Germany), of a platinum rhodium alloy definitely calibrated for a temperature range of 1400°.

Apart from this, these researches contain no essential novelty, except, perhaps, a more detailed attempt to investigate the stem error of the thermometer bulb; their procedure otherwise is identical with the method developed in this country. I am not, therefore, inclined to yield to it the unhesitating deference which has become customary. There can be no doubt, in view of the splendid facilities due to the cooperation of the Royal Prussian porcelain works—facilities which those who have been baffled by porcelain technology, or have had to coax unwilling manufacturers into reluctant compliance, will appreciate—that the data of the Reichsanstalt will eventually be standard. For the present, however, I should be more impressed by some sterling novelty either in the direction of a larger range of measurement or of method. Conceding that an accuracy of 5° at 1000° has been reached, all results above 1500° remain none the less subject to increasingly hazardous surmise.

A beautiful method of absolute thermometry, albeit as yet only partially developed, is due to Töpler. In this the densities of communicating columns of gas are compared very much as in Dulong and Arago's classical methods for liquids, by the gravitation pressures which correspond to these unequally hot columns. To accomplish such extremely fine pressure measurement, Töpler invented the 'Druck libelle,' an inversion, as it were, of the common level, in which, therefore, the motion of the bubble (or of a thread of liquid) indicates a change of pressure conditioned by the invariable horizontality of the instrument.

The development of the practical forms of continuous intrinsic thermoscopes (the *radiation*, the *thermo-electric* and the electric *resistance* methods) went, more or less, hand in hand with the development of the air-thermometer, although the latter is decidedly the more recent. Aside from pioneering experiments of Müller (1858) and others, the well-known Siemens resistance pyrometer (1871) was the first instrument in the field. It was based upon data obtained from platinum, copper and iron by the calometric method of calibration. This instrument has been remarkably perfected by Callendar and Griffiths, using specially pure platinum calibrated by comparison with the air-thermometer as far as about 600°. Notwithstanding these improvements, the resistance pyrometer is inferior, in my judgment, to the thermo-electric pyrometer, from the greater bulk and fragility of the exposed parts and the tendency of platinum to disgregate or waste itself gradually at high temperatures. Its upper limit of temperature measurement is thus limited; for, even if the difficulty of selecting suitable terminals for the coil is set aside, the difficulty of finding an insulator at very high temperatures would remain. According to Holborn and Wien resistance is seriously subject to the influence of furnace gases, and permanence of the low temperature constants does not imply a like permanence of the high temperature constants of the metal.

Radiation pyrometry, curiously enough, is the most venerable method within the whole scope of the subject. It was introduced by Newton (1701) in his *scala graduum caloris* in connection with his well-

known law of cooling. Not to mention minor workers, it was successively attacked and revived in most of the noteworthy high temperature investigations. Pouillet and Draper have studied it; Becquerel, Crova, Violle, Le Chatelier, Langley, Nichols, Paschen and others have advanced it. It remains to-day the most promising as well as puzzlingly fascinating subject for pyrometric research. One needs merely advert to its broad scope in relation to the temperature of the heavenly bodies to acknowledge this. Here I can only allude to Becquerel's principle that the radiation of opaque bodies is spectrometrically alike at the same temperature, a result which has Crova's more recent assent; to Violle's photometric measurements of the total emission of platinum; to the more recent work in the same direction of Violle and Le Chatelier, in which consistent results were obtained for oxide of iron and platinum as far as 1500° to 1700°; to Stefan's law as proved by Boltzmann, and the variety of discussion it has elicited; to H. F. Weber's collateral equation; to the Johns Hopkins measurements, etc. Another school of observers, including Langley, Paschen and others, has undertaken the promising but much more laborious method of bolometric measurement of the distribution of spectrum energy in its relation to temperature. Without doubt, however, the whole subject is yet *in primis rudimentis;* the results are confessedly 'intrinsic.' Indeed, vagueness in the nature of the radiating source lowers with sufficiently threatening aspect to chill the fondest hopes. When one is told by Violle working on Mont Blanc that the temperature of the sun is 2500°; thereupon by Rossetti that it is 9965°, by Le Chatelier that it is at least 7600°, by Paschen that it is below 5000°, by Wilson and Gray that it is 8000°, etc., one wisely concludes that more may yet be learned about it. Our sympathies naturally go with those who, like Lummer and Wien and the Johns Hopkins people, are beginning fundamentally with the search for an absolutely black body. Less superstructure and more sub-cellar is, perhaps, the watchword in radiation pyrometry.

Turning, now, to the last and most important of the methods of practical pyrometry, we find a curiously meandering evolution apparent. I have already indicated that Pouillet (1836) was the first to complete a legitimate piece of calibration work. Pouillet might have condemned the method, but for some reason Tait's thermoelectric anomalies of red-hot iron were not detected. Regnault (1847), who was the next to take up the subject as it happened with the same couple, made his condemnation sweeping enough. It was not the real perversity of the platinum iron couple which provoked Regnault, for of this neither he nor Pouillet became aware. Regnault's objection (as we should put it to-day) lay in the fact that the thermocouple obeyed Ohm's law, which in that early day lay somewhat beyond the great physicist's range of interest. Fortunately, but none the less long after, Becquerel followed with his palladium and divers platinum couples, carefully calibrated and efficiently used. What these platinum couples were is not stated. They cannot have been very sensitive or they would have been preferred to the palladium-platinum couple. Indeed, the metallurgy of platinum alloys did not reach a degree of refinement until Deville and Debray (1875) overhauled the chemical separation of platinum metals with particular reference, both to iridium and to rhodium. Recently Mylius and Förster at the Reichsanstalt further contributed to platinum metallurgy. But in view of the toils in which the whole subject of high temperature measurement languished in Becquerel's day, his results were not sufficient to remove the discredit

which Regnault had thrown upon thermo-electric pyrometry. And so it happened that the return to the method in recent date was of the nature of a resuscitation.

It is amusing to note, as we pass on, the pranks of custom as it bore down upon pyrometry. Following Deville and Troost, every worker (I might mention at least five) felt in duty bound to redetermine the boiling point of zinc—rather a difficult feat in its way. Thus we find boiling zinc inseparably associated with the destiny of the calibrated thermo-couple. Le Chatelier broke this law of fateful sequence by ignoring the need of calibration at the outset, and then using the couple so dignified to determine the melting points of silver, gold, palladium and platinum. But these are Violle's melting points. Hence the pyrometric feature of Le Chatelier's platinum-rhodium couple was in its inception due to Violle.

Meanwhile, accompanying the geologic inquiries of Clarence King, an extensive series of pyrometric investigations which had been in progress in this country since 1882 were completed (1887). These contained a full examination of divers efficient methods of pyrometry and a study of the porcelain air-thermometer with particular reference to the calibration of thermo-couples. In the course of this work the admirable pyrometric qualities of the platinum iridium alloy were exhibited by detailed and direct comparisons with the air thermometer. It was shown that the calibration could be made permanent by referring the thermo-electromotive forces to a Clark's cell; that the character of their variation with temperature is uniformly regular, and that the thermal sensitiveness of the couples increases as the higher degrees of red and white heat are approached. Finally, it was pointed out that couples destroyed by silicate corrosion or in similar ways could be restored by fusing over again on the lime hearth with merely negligible changes of constants. Elsewhere, Le Chatelier's clever combination of the platinum rhodium couple with the D'Arsonval galvanometer, then a comparatively new instrument in the laboratory, secured immediate favor. Professor Roberts-Austen, ever on the watch to waft good things across the channel from Gaul into Albion, hailed the new comer with no uncertain sound. Sometime after, the platinum-rhodium couple entered Germany and was there definitely calibrated (1892) for the first time, as already stated, at the Reichsanstalt.

Of the three available couples, palladium, platinum-rhodium and platinum-iridium, the former is excluded from competition by reason of its low fusibility. Between platinum-iridium and platinum-rhodium, the latter has been more extensively advertised, but is otherwise inferior to the older platinum-iridium alloy. In other words, platinum-iridium, when suitably alloyed, can be made more sensitive than platinum-rhodium in the ratio 100 to 76. Beyond this the alloys are much alike; both are tenacious, resilient, refractory metals, and their thermo-electric forces under like conditions of temperature show a constant ratio even at extreme white heats. The thermo-electric activity of these two alloys is exceedingly remarkable. Among over fifty different platinum alloys examined no similarly sensitive combinations were found. Moreover, whereas platinum alloys of extremely large electrical resistance are not unusual, such metals are not apt to be distinguished thermo-electrically.

To conclude: the small dimensions of the sensitive point of the thermo-couple, the independence of the intermediate temperatures between the junctions (apart from small corrections due to the Thomson effect) and therefore the removal of the terminal difficulty, the high upper limit of the measurable temperatures, the perma-

nence of its constants in relation to Clark's cell in the lapse of time, the instantaneity of its indications, the easy reproduction of destroyed couples, their relative insensitiveness to furnace gases, the regular and simple character of the temperature function, the sustained sensitiveness throughout all temperature ranges even quite into the fusion of platinum—all these facts are a sufficient if not overwhelming recommendation of the method.

In speaking of long range temperature variables one is hardly permitted to overlook the remarkable work which has recently been done in the direction of low temperature, but with these subjects I am less familiar and can therefore only refer to in passing. The progress made in the subject is sufficiently evidenced by the growth of large low temperature laboratories throughout the world, laboratories which undertake 'the cold storage' of 'cold storage,' as it were, like those of Pictet in Berlin and Paris, of Dewar in London, of Kamerlingh Onnes in Leyden, of Olezewski in Krakau, and others. Dewar and Fleming have added to our knowledge of the probable constitution of bodies at the absolute temperature. Olezewski has found the critical temperature of hydrogen at —230° and its atmospheric boiling point at —243°. Dewar and Moissan have liquefied fluorine. There is much here which I must reluctantly forego. The hydrogen thermometer, the platinum balance (Callendar) and the thermo-couple are again doing excellent work in thermometry.

APPLICATIONS OF PYROMETRY.

Turning now to the applications of recent pyrometry, we meet first many series of valuable data on melting points and similarly valuable data on the dissociation temperatures of chemical compounds. To these I merely refer, not being qualified to enter into chemical interpretations. High temperature boiling points have also been treated, and I will especially consider the case of the variation of metallic ebullition with pressure. The relation of vapor pressure to temperature has thus far defied the counsels of the wise, even though such men as Bertrand and Dupré have given the matter close scrutiny. One would suspect the simplest relation to hold for metallic boiling points, and investigations have therefore been undertaken in which the temperature of ebullition of Hg, Cd, Zn, Bi, were studied for pressures decreasing from one atmosphere down indefinitely.

The results so obtained show an effect of pressure regularly more marked as the normal boiling point is higher, so that the attempt to express the phenomenon for all these bodies by a common equation is roughly successful. By far the most rapid reduction of boiling point occurs when the pressure decreases from $\frac{1}{10}$ atmosphere indefinitely. For the case in which the normal boiling point is to be predicted from a low pressure value in case of a metal which, like bismuth, boils with great difficulty, very high exhaustion is essential.

Igneous fusion, by which I mean the fusion of rock-forming magmas, is particularly interesting in its relation to pressure. This has been again recently pointed out by Clarence King in his discussion of the age of the earth. If the earth is solid within, as is now generally admitted, such solidity can only result from superincumbent pressure withholding fusion. To study the relation of melting point to pressure directly is out of the question when white heat is the condition of fusion. In this respect the laboratory in the interior of the sun or even of the planets has some salient advantages, but we cannot comfortably put such a laboratory under strict surveillance of protoplasm.

Fortunately, the Clapeyron equation, successively improved by James Thomson and

by Clausius, is here usefully available. To measure the melting point, the difference of specific volumes of the solid and the liquid body and the latent heat of fusion at this temperature, with the aid of Joule's equivalent, is to measure also the relation of melting point to pressure implicitly. Based on the first and second laws of thermodynamics, this equation is generally true, no matter what specific properties may characterize the body. The process has thus far been completely pushed through for diabase only. Thermal change of volume may be measured by enclosing the rock in a platinum tube of known expansion, and the contraction of the contents from liquid to solid found by an electric micrometer probe reaching within the tube. Given a furnace fully under control, then, as experiment has shown, the cooling can be made to take place so slowly that platinum remains rigid relatively to its charge of red-hot magma, and under these conditions the contraction can actually be followed into the solid state. At the same time the temperature at which marked change of volume occurs is the melting point. Apart from difficulties of manipulation, the latent heat may be found from measurement of thermal capacity on either side of the temperature of fusion by a modification of known methods.

The rate at which fusion is retarded by pressure computed from these data in the manner specified showed an increase of the melting point of a silicate of about 0.025°C. per superincumbent atmosphere. But this datum falls within the margin (.02 . . . .04) of corresponding data much more easily and directly derived for organic bodies. One may, therefore, argue that if the melting point pressure rate is so nearly constant on passing from the class of silicious to the thoroughly different and much more compressible class of organic bodies the rate would probably be more nearly constant in the same body (silicious or organic), changed only as to temperature and pressure. This surmise was verified for naphthalene within an interval of 2,000 atmospheres.

The endeavor to interpret the change during fusion of the volume of the chemical elements in terms of the periodic system has been begun with much success by Max Töpler for low temperatures. It would be of great interest to complete this diagram for high temperatures in view of the specifically molecular character of the fusion phenomenon, by repeating such experiments as have just been described for rock magmas.

The heat conduction of rocks has been investigated in many cases for temperatures lying below red heat. Among recent observers we need only instance the extensive investigations of Ayrton and Perry. No trustworthy experiments, however, have yet been carried into the region of essentially high temperature; and yet what is chiefly of interest in the geological applications of such experiments is the change of conduction which accompanies changes of physical state, whether induced by pressure or by temperature.

Experiments in heat conduction are difficult under any circumstances. They become insuperably so when conduction at white heat is to be studied under pressure, and that is what the geological conditions actually imply. Some notion of a body respectively solid and liquid at a given temperature may be obtained by observing the behavior of bodies which are capable of being undercooled. A number of such bodies are known, thymol being a conspicuous example. Experiments with this body were made by measuring the volume expansion, specific heat and heat conduction in parallel series both for the solid and liquid state at like temperatures. They showed, for instance, that the increment of

absolute heat conduction, encountered in passing isothermally from the solid to the liquid state, when referred to solid conductivity is about 15%, and when referred to a liquid conductivity is about 15%. Similarly the change of thermometric conductivity under like conditions is an increment of 36% and 56% respectively. Now, since in most questions relating to thermal flow thermometric conduction enters exclusively, the importance of this large coefficient is obvious whenever a body passes from the solid to the liquid state.

Solid conduction is thus 40 or 50% in excess of liquid conductivity for the same body at the same temperature and pressure. It is reasonable to infer that a corresponding decrement of conduction will accompany any rise of temperature of a solid body. Measurements which have somewhat recently been made for relatively small intervals at Zurich, at Glasgow and at Harvard, upon typical rocks, all bear out this surmise. The immediate incentive to these experiments was a strong paper by Professor Perry in which Lord Kelvin's estimate of the age of the earth is shown to be insufficient for an earth in which the interior conductivity is systematically greater than the surface conductivity. Indeed, he deduces the percentage increment of the square root of the age of a Perry earth over that of a Kelvin earth to be one-fifth of the percentage decrement of conduction for each 100° C. So far as the effect of terrestrial temperature alone is concerned, the measurements just mentioned show that Perry's correction is negative or that Perry's earth would be less long-lived than the 100×10^6 limit of years set by Lord Kelvin.*

To estimate the effect on heat conduction of the increase of pressure which accompanies the increase of temperature with the depth below the surface is a much more serious matter. In the laboratory, pressure experiments are limited to 3,000 or 4,000 atmospheres; compared with earth pressures these scarcely amount to a scratch on the surface; yet even for this limit the determination of heat conduction at high temperatures is out of the question. A tentative method of arriving at a conclusion is given by Clarence King in his discussion of the age of the earth, the consequences of which have been quite overlooked. What King endeavored to accentuate long before Perry's contribution to the subject was precisely the fact we *cannot* assume greater conductivity for the interior than for the surface. Since heat conduction decreases isothermally from solid to liquid, it is assumed that in one and the same substance the viscosity could be taken as an index of the thermal conduction. Therefore, if temperature and pressure were made to vary in such a way (both increasing) as to leave viscosity constant, it was inferred that heat conduction would also remain constant. Now the isometrics or lines of constant viscosity of a viscous body for variable pressure and temperature are much more easily found than the isometrics of conduction. In fact, it has been shown that a burden of at least 200 atmospheres would have to be brought to bear in order to wipe out the decreased viscosity due to the rise of a single degree (Centigrade) of temperature. The depth at which this ratio is reached, as King points out, for a given surface gradient of temperature and depth, depends on the initial excess of the temperature of the earth considered, and on the age of the temperature distribution resulting. But no matter whether the Kelvin earth with an initial excess of 3,900° and an age of 100×10^6 years, or whether King's solid earth with an initial temperature of fused platinum and 25×10^6 years of life be taken, in all cases the temperature effect predominates

*The text of Kelvin's recent address at the Victoria Institute, in which an age of thirty million years is maintained, has not yet reached me.

throughout those depths within which change of temperature with depth is the marked feature of the temperature distribution. In other words, if, for example, we consider the Kelvin earth, the strata above .035 of earth radius will be strata of smaller conduction than the surface strata. From the surface downward as far as .035 radius, conduction will decrease to a minimum. Below this, conduction will increase again, due to preponderating pressure, finally to exceed the surface value. But the computed temperature distribution of Kelvin's earth is such that at .035 radius the initial temperature excess of 3,900° has been reached to within one or two per cent. Below this in depth Perry's correction will begin to apply, but the further changes of temperature are so nearly negligible that the consideration of conduction is superfluous. From this point of view, therefore, the staggering force of Perry's clever argument is removed. Of course, I am fully aware that an argument from the supposed parallelism of physical properties of a given body (in the present case heat conduction and viscosity) is not inevitably convincing; but in physical geology, for some time to come I dare say, the question will be not one of rigorous proof, but rather one of forming a rational opinion.

In passing I will indicate the importance of an increased knowledge of the isometrics of liquid and solid matter, relations which have thus far been found simpler in character than other thermo-dynamic properties as I shall again point out in the course of the address.

I want finally to add a few words on the electro-chemistry of magmas. The physical chemistry of molten rock has already been somewhat extensively considered, but I am hardly competent to review it. In this country, Joseph Iddings and, more recently, George F. Becker have discussed the natural differentiation of magmas from different points of view. Here I will merely include certain pyrometric experiments on the electric conduction of fused glasses which seem to give promise of throwing light on the chemical constitution of complex silicates and to be suggestive in other ways. In such measurements, if the magma is made to pass from the solid to the liquid state, the observed electric conduction contains no evidence either of a melting point or of polymerism. The law of thermal variation is easily derived and it agrees closely with the corresponding behavior of a zinc sulphate solution, for instance, observed through a range of temperature. In a general way, electric resistance decreases in geometric progression when temperature increases in arithmetical progression. Considered relatively to the composition of the magmas, electric conduction shows a marked and regular increase with the degree of the acidity of the magmas. The less fusible acid magmas are better conductors than the basic magmas at the same temperature. Curiously enough, conduction thus runs in an opposite direction to fusibility. However viscous a magma may be, therefore, and however cogent the arguments, such as those launched by Becker against the differentiating importance of ordinary diffusion may prove, it is fair to conclude that a thorough change of chemical structure through *ionic* diffusion, whether directed by an electric field or otherwise, must be an easy possibility for a sufficiently hot but otherwise solid magma. The results point specifically to the desirability of repeating Hittorf's brilliant experiments on the migration of the ions for a silicious medium. This ought not to be difficult, seeing that such a menstrum need not even be liquid to be compatible with a high order of electric conduction.

Further consideration of the subject shows the probable passage of conduction through a maximum when acidity is con-

tinually increased. Finally quartz appears like an insulator in the same rôle as water in ordinary aqueous solutions. In all these cases I wish to keep in mind the results of Alexeëff and their recent repetition for metallic alloys, together with the interpretation of these results due to Masson. In a crust subject to variable magnetism, traversed by earth currents, sustained by semi-metallic carbides of the Mendeleëff-Moissan type, containing piezo-electric and thermo-electric sources, who can say that electric fields are absent? Again, the character of the changes contemplated in Gibbs' famous 'phase rule,' as interpreted by Le Chatelier, would here be ionic rather than molecular.

A question of somewhat allied interest in the action of hot water under pressure on rock-forming silicates. Investigations of this kind have been described in the well-known and fascinating book of Daubrée. Daubrée's work, however, is qualitative in character, like that of many others in the same line, and the furtherance of the subject is to be looked for in the quantitative direction. Some time ago Becker suggested experiments on a huge mass of granulated rock under the action of steam at exceptionally constant temperature. But no thermal effect of the action of water could be detected. True, the boiling point of water is a temperature comparatively low for the purpose; yet similar experiments made with liquid water at over 200° under pressure were equally negative as to results. Experiments of this kind are not very conclusive. The insufficient sensitiveness of the measuring apparatus, the rate at which heat is carried off compared with the rate of generation and other obscure causes mar the results. The question may, however, be approached in a somewhat different way: If water is heated under pressure in glass tubes, the volume of water contained decreases as the square, whereas the chemically active area, *i. e.*, the inside surface of the tube, decreases as the first power of the diameter. Hence, in proportion as the tube is more capillary, the action of water on the glass will produce accentuated volume effects. Thus it was shown that the behavior of hot water is profoundly modified by its continued action on glass, inasmuch as its compressibility increases at a very rapid rate with the time of action even at 180°, until, with the approach of solidification, the observed compressibility is fully three times its isothermal value at the inception of the experiment. Even more striking is the simultaneous and continual decrease of the length of the column of water. Clearly, therefore, the confined volumes of glass and included water must undergo contraction at 180° in forming an eventually solid aqueous silicate, while increasing compressibility is due to the increasing quantity of silicate dissolved. Now, in nearly all cases the effect of solution is a decrease of compressibility. Hence the increased compressibility observed is to be referred to a precipitation of the dissolved silicate, in response to the action of pressure, a result borne out by the appearance of the tube and by varied correlative experiments. It is, however, the volume contraction which is particularly interesting, because of its far-reaching geological application. In the first place the measurements show that about .025 cubic cm. of liquid water is absorbed per square centimeter of glass surface at 180° C. per hour.* The effect of this absorption is a contraction of bulk amounting to 18% per hour. So large and rapid a contraction is presumably accompanied by the evolution of heat. Hence, under conditions given within the first five miles of the earth's crust, *i. e.*, if water at a temperature above 200° and under sufficient pressure to keep it liquid be so circumstanced that the heat produced

* This is an initial rate of about 180 kilograms per square meter per year.

cannot easily escape, the arrangement in question is virtually a furnace whose efficiency accelerates with rise of temperature or increase of terrestrial depth.

PIEZOMETRY.

It is not feasible to make much progress in pyrometry without feeling the need of a corresponding development in high pressure measurement. This has already appeared in the preceding parts of my address. It will not be expedient to look into the history of the subject as comprehensively as I did in the case of pyrometry, partly because the literature is more diffuse, and partly because the real development of piezometry is of recent date and virtually begins with pressures of the order of several thousand atmospheres. So understood, although we gladly pay homage to Oersted, to Regnault, to Grassi and many others, our historical summary may be abridged.

As is often the case in physics, the great advances in the subject are associated with the name of one man; for, though many able investigators have contributed effectively to the progress of piezometry, the overshadowing importance of the results of Amagat have superseded all researches coextensive with his own. For over twenty years Amagat has been laboring on this definitely circumscribed subject. Year after year his prolific experimental ingenuity has put forth results, each of which in its turn constituted the highest attainment in accuracy and the greatest breadth of scope which high pressure measurement had reached at the time. It is impossible to give any adequate view of this sustained labor in an address. The subject is highly specialized and demands special treatment; but we owe to Amagat the bulk of our knowledge of the properties of a gas regarded not as an ideal fluid, but as a physical body; some of the most far-reaching results in the thermo-dynamics of liquids, and some of the best data in the elastics of solids.

Amagat investigated gases within an interval of pressure which at times reached 4,000 atmospheres, with a view to interpreting their divergence from the laws of ideal gaseity. Indeed, we may note in passing that just as the advanced astronomy of the day is being enriched with unexpected discoveries from a discussion of mere errors of observation, so refined physical measurement gleans new harvests in carefully tracing out the all but rigorous sufficiency of established laws. The product of pressure and volume, nearly constant in the ordinary isothermal behavior of gas, shows, under higher pressures, a well-marked passage through a minimum in the case of all gases except hydrogen. Hence, below a certain definite pressure, varying with the character of the body (say 40 atm.), gases are more compressible than Boyle's law asserts, and above this pressure they are continually less compressible and begin to resemble hydrogen in this respect. The sharpness of the minimum diminishes as temperature increases, and probably ultimately vanishes. Cailletet, it is true, had undertaken a study of the same subject simultaneously, but his results were not in the same degree correct. Again, the coefficient of expansion of gases, considered in its isopiestic behavior for temperatures not too far above the critical point, increases with pressure to a maximum, which seems to occur at the same pressure for which the volume-pressure product is a minimum. This thermal maximum also decreases with temperature and finally vanishes. To specify the conditions further than this would be to exceed the limits beyond which verbal statement ceases to be lucid. The value of Amagat's work, however, is not merely the formulation of such general laws for gases as a whole, but rather the investigation of sharp and specific results for each gas individually. Thus,

if one uses these data for a given gas to compute the contestants in Van der Waal's law, one is actually able to predict remote critical conditions of the gas in question with a fair order of accuracy.

Whenever pressure measurements are to be made through such large intervals as are here in question, the elastic constants of the apparatus become of increasing moment. Amagat, however, treated these incidental measurements as of like importance with the rest of his labors. The starting point of his investigation into high pressures was the open mercury manometer first erected along a staircase near Lyons, finally in the shaft of the St. Etienne mine, about 380 meters deep. This apparatus was used for graduating the closed manometer, preferably containing nitrogen. In later experiments for excessively high pressures the closed manometer was replaced by the 'manomètre à pistons libres,' a sort of inverted Bramah press, in which the small pressures of an open mercury manometer acting on a large piston compensate the relatively large pressures of the piezometer acting on a small piston. The ingenious feature of Amagat's apparatus is the rotation of both pistons just before measurement, a device by which friction is rendered harmless. Equipped with this instrument direct determination of the bulk modulus for glass and metal was actually feasible. In the case of glass no serious variation of the compressibility could be ascertained within 2,000 atmospheres, and even 200°, an observation of great value in practical research. Poisson's ratio was similarly determined and the data used in computing Young's modulus. But the most important result of these researches, a result to which Professor Tait also contributed, is the datum found for the absolute compressibility of mercury. This will enable all future observers in piezometry to standardize the apparatus with ease and nicety.

Time prevents me from dwelling upon the remaining investigations of Amagat in a measure commensurate with their value. These contain a counterpart for the liquid state of the results already announced for gases. The change of volume throughout enormous pressures and about 200° of temperature is considered in detail for a number of important liquids. Only in one case, and that the rather remarkable one of carbon tetrachloride, are evidences of solidification encountered and the conditions determined. Amagat believes the absence of solidification to be due to the occurrence of the lower critical temperature below the isothermal of compression. In my own judgment, however, the pressures necessary to reach this lower critical point will be enormous even in units of 1,000 atmospheres, for which reason it is not in any case liable to be an easy conquest.

Special mention finally is due to the thermal position of the maximum density of water, which Tait had already studied. Amagat shows definitely that the temperature of maximum density moves towards the freezing point with increasing pressure, so that at high pressures, as well as at high temperatures, the behavior of water loses its anomalous character. In general, compressibility and expansion decrease with pressure for all normal liquids, and expansion increases rapidly with temperature. Other anomalous properties of water have been investigated, among which the diminished viscosity of water under pressure at ordinary temperature studied by Röntgen, Cohen and others may be stated.

After this cursory and wholly inadequate mention of the work of Amagat and the physicists who, like Tait, Cailletet and others, have been engaged in closely allied researches, it will repay us to look at some of the other as yet less splendidly developed contributions to piezometry. At the outset it is well to make mention of the forms of

pressure gauges which have come into use. As far as 1,000 atmospheres, the Bourdon gauge, if well constructed, does good service, though in a somewhat rough way. The corrected nitrogen closed manometer is more accurate for a smaller range. A theoretically simpler pressure gauge was proposed by Tait and Cailletet. In this case a straight cylindrical elastic tube under internal or external pressure is substituted for the Bourdon tube and the expansion or compression is directly measured. With due precautions against changes of temperature and the choice of a solid of constant bulk modulus and rigidity, this gauge can be used as far as about 2,000 atmospheres with convenience.

Above 2,000 atmospheres Amagat's Bramah press manometer, already mentioned, is the only reliable gauge, and, though somewhat cumbersome, has the advantage of giving absolute results. However, a gauge based on the change of electric resistance of mercury with pressure, a constant now fairly well known from Palmer's measurements, will, in my judgment, do good service for pressures which exceed even the range of the manometer. With regard to methods for producing high pressures, the force pump, with a small steel plunger and the screw advancing bodily into a closed barrel filled with a liquid, have not yet been superseded. The efficiency of such apparatus depends essentially on the means used for obviating leakage. These must, of course, be very perfect.

Amagat's work with liquids was extended chiefly in the direction of high pressures Experiments have since been made by others throughout higher temperatures (310°) and, of course, a smaller range of pressures (500 atm.). Leaving out the less perspicuous results, I will here merely allude to the probable existence of a remarkable law which these researches have developed. Dupré (1869) and afterwards Lévy (1878), reasoning from thermo-dynamic premises, were the first to suspect that the isometrics or lines of equal volume of liquids are straight. In other words, if there is to be no change of volume then pressure increments must vary proportionately to the temperature increments ($p = a\theta - b$), a result which implies that the internal pressure of a body kept at constant volume is proportional to its temperature. Lévy's deduction was, however, declared to be theoretically unwarrantable by Clausius, Boltzmann and others. Sometime after, the same law reappeared in experimental form in a series of brilliant researches on critical temperatures due to Ramsay and Young. Fitzgerald, reasoning from Ramsay and Young's results, then proved that for such liquids as possessed straight isometrics specific heat is a temperature function only, and energy and entropy are each expressible as the sum of a mere temperature function and a mere volume function. This is curiously like the position from which Dupré and Lévy started. Ramsay and Young's work, however, applied specifically to vapors, and for high temperatures (200°) their pressures did not exceed 60 atmospheres. The law has since been tested for liquids as far as 1500 atmospheres and over 200° conjointly, and found in reasonable accordance with experiment. Hence we infer that if the thermo-dynamic change of a body is such that volume remains constant, pressure and temperature will vary linearly with each other, the increments being about 0.1° C. per atmosphere. Now, although any law relating to the liquid state would be welcome, these volume isometrics are particularly so. In the geology of the earth's crust, for instance, they would, in a great measure, determine the conditions of possible convection; and it is curious to note that, from the known values of bulk modulus and of the expansion of solid glass, the isometrics

would here again be given by corresponding increments of about .1° per atmosphere. For solid metals the isometrics are of a different order.

Another line of research for liquids to which I attach supreme importance has only just been begun: I refer to the systematic study of the *entropy* of liquids. Among the first results on the heat produced in suddenly compressing a liquid are those of Tait. They are of too limited a range, however, and not in good accord with the more recent and extended data of Galopin. Generally speaking, the change of temperature produced per atmosphere of compression increases with temperature in a marked degree, as one may infer from Kelvin's equation. For organic bodies this increment at ordinary temperatures is of the order of $\frac{1}{50}° = .020°$ per atmosphere. In case of liquid metals the order of values is decidedly different, being about $\frac{1}{10}$ this value, recalling correspondingly divergent results observed for the isometrics of volume. Quite recently (1896), the same subject has been taken up by Tammann (to whom we also owe results for the correlative compressibility) particularly for solutions and with reference to the theory of solutions. Tammann's data are of the order .001° per atmosphere at 0°, and in better keeping with the thermo-dynamics of the subject than the earlier experiments. Much more, however, must be done before anything like a degree of critical accuracy is approached or a broad survey taken. Pressure intervals are to be chosen wider and the temperature measurement given with greater certainty.

Finally, I wish to touch upon the relations of melting point and pressure in their more recent development. Obviously, the classical work of Andrews on the continuous passage of a liquid into the gaseous state will find some counterpart in the manner in which the analogous passage from the solid into the liquid state takes place. The character of these phenomena may be shown from direct observations of melting point and pressure, as was done by the earlier observers. Full knowledge, however, can be obtained only by mapping out the isothermals throughout the region of fusion very similarly to the method pursued by Andrews himself for vaporization. This has thus far been attempted for a single body only, naphthalene, within 130° and 2,000 atmospheres. Six isotherms (63°, 83°, 90°, 100°, 117°, 130°) were traced within these intervals, along each of which, excepting the first, the body passed from the liquid to the solid state under the influence of pressure only. An exhibit of these data shows strikingly that in all cases the change of physical state takes place in accordance with a cyclic law, *i. e.*, a larger pressure is necessary to change the body from the liquid to the solid state, at a given temperature, than the pressure at which the body at the same temperature again spontaneously melts. Freezing almost always seems to take place at once; the corresponding fusion is apt to be prolonged, and in its gradual occurrence traces the contours of James Thomson's well-known, doubly-inflected isothermals much more fully than does the allied case of vaporization.

The appearance of the cyclic parts of these isothermals is suggestive, and may be described in terms of their dimensions in the direction of volume and of pressure respectively. The former dimensions indicate the probable fate of the volume increment. They show that, as pressure and temperature increase, the volume increment tends more and more fully to vanish, and they thus imply a lower critical temperature at which the solid would change into the liquid continuously as far as volume is concerned. It does not follow that other properties of the body would here also vary

continuously. For naphthalene this point would lie in a region of several thousand atmospheres and several hundred degrees Centigrade—therefore, in a region too remote to admit of actual approach.

Again, the breadth of the cycles, measured along the pressure axis, decreases from the center of the field both in the direction of increasing and decreasing pressures. The tenor of these results is an additional indication of the recurrence of a lower critical temperature at which cycles must necessarily vanish. The decrease of the breadth of the cycles in the direction of decreasing pressure suggest the possible occurrence of a point in the region of negative external pressure, so circumstanced that beyond it the substance would solidify at a lower pressure than that at which it fuses. This may be interpreted as follows: The normal type of fusion changes continuously into the ice-type of fusion through a transitional type characterized by the absence of volume lag.

An independent discussion more searching in character has quite recently been given by Tammann. Tammann points out that for the normal case of fusion and for increasing pressure the two determinative factors of the Clapeyron equation—the volumes and latent heat of fusion—will not in general simultaneously become and remain zero. He argues that the volume constant will at the outset decrease with pressure passing through zero to negative values. Hence the curve representing the relation of melting point to pressure must initially rise to a maximum when the melting point pressure ratio is zero, and then decrease. Contemporaneously, the latent heat of fusion, decreasing continually with pressure, eventually also reaches zero, but at a much later stage than the volume constant. At this stage, therefore, since the melting point and the volume constant now have definite values (the latter negative), the melting point and pressure ratio is negatively infinite. Hence the curve expressing the relation of melting point to pressure decreases with increasing pressure from the maximum specified, as far as the pressure at which latent heat is zero, and there drops vertically downward. Thus Tammann's melting point pressure curve, with its initial and final ordinate in the direction of temperature, maps out a field of pressure and temperature within which the body is solid. Outside of this region the body is liquid and cannot by pressure alone be conceivably converted into the solid state. Any thermo-dynamic change involving a march through the boundary of this region is accompanied by the discontinuity of fusion, of viscosity, etc. A march through the final ordinate (for which latent heat is zero) is probably not accompanied by such discontinuity. For a given temperature there may be two fusion pressures. At a temperature sufficiently below the melting point the continued increase of pressure should, therefore, move the normally fusing body from the solid into the liquid state continuously. This is a somewhat anomalous result of close reasoning, but it must not be forgotten that in the depth of our ignorance of the actual occurrences above several thousand atmospheres the term anomaly is a misnomer. Indeed, if we regard the melting pressure curve beyond the stated maximum as characterizing the ice type of fusion (which Tammann does not do) our difficulties would in a measure be reconciled.

Tammann finally points out that the term lower critical temperature is not justified by the character of the phenomenon. Data for melting point and pressure due to Damien seem directly to corroborate the occurrence of zero values in the ratio of melting point and pressure increments, but Damien's tests are restricted to a pressure interval much too small to be trustworthy.

Of the two bodies which have been tested throughout long pressure intervals, naphthalene shows a linear melting point and pressure ratio for 2,000 atmospheres, while the carbon tetrachloride of Amagat, though initially concave upward, soon also becomes linear. Clarence King has, therefore, in geological considerations so represented it. To conform with Tammann's inferences the interior of the earth would have to be a fluid.

One point of issue, however, in these cases is clear: At Andrews' critical temperature both the difference of specific volumes and the latent heat of fusion vanish simultaneously wherever observed. Under corresponding conditions of change from liquid to solid the internal pressures are of tremendously greater value for both states, and the passage of the solid into the liquid molecule may involve an immense transfer of energy without any corresponding change of volume, for the density of the molecule itself eludes observation. The manner of this isothermal change from one state to the next is in all cases along the characteristic doubly-inflected contour first pointed out by Thomson for vapors, and since elaborated by Van der Waals, Clausius and others. We may, for brevity, call this a *volume lag* and measure it in terms of the pressure or the volume interval subtended. The liquid can exist even above the critical temperature, which would mean that even here pressure must be reduced below the critical pressure in order to rupture the liquid molecule.

Pronounced as these phenomena are for the change from gas to liquid, they become much more remarkable, indeed often formidable, for the change from liquid to solid. In this case a volume lag subtending more than 100 atmospheres is the rule. In other words, it takes much greater pressure to solidify a liquid at a given temperature than to liquefy the solid. Among all these cases there is a group of well-known bodies in which, while the solidification pressure is of marked intensity, the isothermal pressure of spontaneous fusion may even be below zero or be in the region of negative pressure. Take the single example of thymol among many: This body between zero Centigrade and its melting point at 53° can be kept in either the solid or the liquid state of pleasure. Given at about 50° in the liquid state it would require more than 2,000 atmospheres to solidify it. If solid it must obviously remain so even if pressure be wholly removed. But thymol may be similarly treated beginning with the undercooled liquid state at 28°, *i. e.*, 25° below its melting point. Even here at least one thousand atmospheres are needed to condense it (400 have been tried quite ineffectively). Once solid it would require about 1,000 atmospheres of *negative* external pressure again to melt it. In other words, it could not be melted again on the same isothermal.

If we but knew more about the physical constants involved in these transformations we could predict the results along the lines of J. W. Gibbs' splendid theory of the equilibrium of heterogeneous mixtures; but, with the dearth of our concrete knowledge of long range physical phenomena relating to liquids and solids, we must be content with humbler methods.

I have always regarded the significant behavior instanced for the case of thymol as capable of a broad interpretation. Professors J. J. Thomson and Fitzgerald abroad, and Elihu Thomson in this country, have recently sought for atomic dissociation in the electrolyzed vacuum of a Crookes' tube. Speaking to the same point, I would venture to assert that we may reasonably look to the volume lag for a rational account of the genesis of atoms. We have already met with two orders of volume lag: the first at the mergence of gas into liquid being usually a few atmospheres in isothermal

value; the second at the mergence of liquid into solid, a hundred or even one thousand times as large in isothermal value, and characterized by the fact that, whereas freezing pressures may be enormous, the corresponding isothermal melting pressure may even be markedly negative.

If, then, we further inquire as to what will happen if we indefinitely compress the solid along a suitable isothermal, I think it is logically presumable that, with the succeeding and profoundly accentuated volume lag, we shall reach the next atom in a scale of increasing atomic weights.

However enormous the condensation pressure for this purpose may be, it is supposable, in the light of the examples already given, that along an accessible isothermal the disintegrating external pressure of the new atom may be permanently negative. Hence the new atom will persist within the pressure and temperature range available in the laboratory.

But the last stage is virtually identical with the first, or the inherent nature of these changes is periodic. The inference is therefore that, under suitable thermal conditions and continually increasing pressure, the evolution of atoms, of molecules, of changes of physical state, again of atoms and so on indefinitely, are successive stages of periodically recurring volume lag.

Carl Barus.

Brown University.

THE AMERICAN MATHEMATICAL SOCIETY.

The American Mathematical Society held its fourth annual meeting at Toronto, Canada, on Monday and Tuesday, August 16th and 17th. All conditions were exceptionally favorable for the success of the meeting, but the result far exceeded all anticipations. Forty-four members of the Society were registered as in attendance, forming by far the largest body of mathematicians ever brought together on this continent. Among those present were Professor Simon Newcomb, President of the Society, and former Presidents Dr. G. W. Hill and Dr. Emory McClintock. Several distinguished visitors also attended the meeting, among them Professors A. R. Forsyth, A. G. Greenhill and O. Henrici. Twenty-three papers were presented, all by members of the Society. Although the great length of the program necessitated a severe limitation of time for the reading of each paper, the hearty cooperation of the authors with the plans of the committee enabled the Society to conclude the sessions in the two days allowed without the least friction. The meeting was characterized not only by great scientific interest, but also by a cordial spirit of good feeling between hosts and guests.

At the meeting of the Council on Monday evening three persons were elected to membership in the Society and three nominations were received. It was decided to hold the next summer meeting of the Society at Boston, Mass., at or about the time of the meeting of the American Association for the Advancement of Science. Resolutions were adopted by the Society in recognition of the generous hospitality of the University of Toronto and its officers.

A portion of the afternoon session on Tuesday was devoted to a general discussion of the topics mentioned below. The members of the Society were afterward received by President and Mrs. Loudon, of the University of Toronto. On Tuesday evening the members of the Council were entertained at the Toronto Club by Professor Alfred Baker.

The following is a complete list of the papers presented:

MONDAY MORNING.

1. *Upon the representation by ruled surfaces of the curves drawn by mechanisms. Preliminary report, illustrated by models:* Dr. Edwin M. Blake, Purdue University.

2. *A contribution to the theory of plane curves:* DR. L. WAYLAND DOWLING, University of Wisconsin.

3. *A Canadian discoverer of the non-Euclidean Geometry:* PROFESSOR GEORGE BRUCE HALSTED, University of Texas.

4. *Note on the Folium of Descartes:* PROFESSOR ELLEN HAYES, Wellesley College.

5. *A geometrical locus connected with a system of coaxal circles:* PROFESSOR THOS. F. HOLGATE, Northwestern University.

6. *On the solution of the map-color problem:* PROFESSOR P. WERNICKE, State College of Kentucky.

7. *On the Riemann-Helmholz-Lie problem of the foundations of geometry:* PROFESSOR H. B. NEWSON, University of Kansas.

MONDAY AFTERNOON.

8. *Quaternion invariantive operators:* PROFESSOR JAMES BYRNIE SHAW, Illinois College.

9. *The geometry of some differential expressions in hexaspherical coordinates:* DR. VIRGIL SNYDER, Cornell University.

10. *On certain differential equations of the second order allied to Hermite's equation:* PROFESSOR E. B. VAN VLECK, Wesleyan University.

11. *Concerning the cubic involution and the cubic transformation of elliptic functions:* PROFESSOR OSKAR BOLZA, University of Chicago.

12. *The determination of the rational function in the reduction of the general Abelian integral to the sum of a rational function and a fundamental system of elementary integrals:* DR. J. C. FIELDS, Berlin, Germany.

13. *On the reduction of hyperelliptic functions ($p=2$) to elliptic functions by a transformation of the second degree:* D. J. I. HUTCHINSON, Cornell University.

TUESDAY MORNING.

14. *Further researches in the theory of quintic equations:* DR. EMORY MCCLINTOCK, New York, N. Y.

15. *A theorem concerning the coefficients of lineal substituting groups of finite order with n variables:* PROFESSOR H. MASCHKE, Chicago University.

16. *On the commutator groups:* DR. G. A. MILLER, Ann Arbor, Mich.

17. *Collineations in a plane with invariant conic or cubic curves:* PROFESSOR H. S. WHITE, Northwestern University.

TUESDAY AFTERNOON.

18. *Concerning regular triple systems:* PROFESSOR E. H. MOORE, University of Chicago.

19. *Theory of discrete manifolds:* MR. F. W. FRANKLAND, New York, N. Y.

20. *Certain transformation problems of canonical equations of dynamics:* DR. EDGAR ODELL LOVETT, University of Chicago.

21. *The true transition curve:* MR. P. H. PHILBRICK, Lake Charles, La.

22. *About sixth power numbers whose sum is a sixth power:* DR. ARTEMAS MARTIN, Washington, D. C.

23. *Preliminary report on alternate functions of complex numbers:* PROFESSOR A. S. HATHAWAY, Rose Polytechnic Institute.

General discussion of the following topics:

1. *The accurate definition of the subject-matter of modern mathematics.*

2. *The vocabulary of mathematics. The possibility of correcting and enriching it by cooperative action.*

F. N. COLE,
Secretary.

CURRENT NOTES ON PHYSIOGRAPHY.

ARTESIAN WELLS OF IOWA.

A COMPREHENSIVE report on the artesian wells of Iowa by W. H. Norton (Iowa Geol. Surv., VI., 1896, 113–428) brings clearly forward the favorable conditions there obtaining for this important source of water supply. The paleozoic strata have a gentle southward inclination, and also a sag over the northern part of the State from the eastern and western boundaries towards a median line. Numerous water-bearing strata, or *aquifers*, occur, from the basal sandstone that lies on the buried hills and valleys of the Algonkian floor to the sandstones of the coal measures. Cretaceous strata in the northwestern part of the State lead water away westward. Some important wells are supplied from aquifers in the glacial drift. Three artesian wells were mentioned in the State Survey Report for 1870, and no systems of water works had been then constructed. About a hundred systems are now in operation, many of them depending on artesian supply. At certain points, where many wells have been sunk, the first yield has slightly decreased, as if the limit of local supply had been reached; but great stores of subterranean water remain unexhausted beneath most of the State.

FORESTS AND DESERTS OF ARIZONA.

THE variation of climate with altitude

and the redemption of the desert along irrigated valleys afford material for an entertaining narrative by Fernow (Nat. Geogr. Mag., VIII., 1897, 203–226). Much of the lower ground is desolate, unless exceptional rains allow a temporary growth; but the few valleys of streams large enough to feed irrigating canals contain green groves, orchards and fields. The higher plateaus and volcanoes bear forests of open growth; about Flagstaff 12,000,000 feet of lumber are cut annually. The open grouping of the trees is ascribed to variation in water storage by the soil. Although millions of pine seedlings spring up after the rains, all of them perish from frost, unless the season is exceptionally favorable. The young tree growth does not follow in annual generations, but in irregular periods of from three to ten years. Open grassy glades of fine black loam seem to mark the site of extinguished lakes. Brief account is given of an ascent of San Francisco mountain and of a visit to the Grand Canyon of the Colorado, but it seems rather negligent of the rest of the world to speak of even the unrivalled canyon as 'a thought of God on earth expressed.'

TRANSVERSE VALLEYS IN THE JURA.

A NEW contribution to the above much-discussed question is made by Dr. Fr. Jenny, (Das Birsthal: ein Beitrag zur Kenntniss der Thalbildung in Faltengebirge, Basel, 1897), with the conclusion that the several transverse valleys or *cluses* of the Birse are the result of backward erosion along lines of local distortion in the anticlinal ridges, whereby the river course has become more or less adjusted to the structure of the region. It is argued that in the early stages of folding, the drainage was consequent, and that several temporary lakes were then formed. Local deformation in the growing arches defined the river course. "The Birse has gained its present course by a change from its original path through adjustment to the structure of the mountains that it traverses."

It is not clear whether the author advocates the diversion of early consequent streams to transverse courses by the headwater growth of diverting streams or not; no explicit statement being made on this point. The antecedent origin of the rivers, advocated by Foerste and Rollier, is discarded. Yet a qualified antecedent origin is implied, for the present river course is given an early date, before the folding was nearly completed; as if consequent on the early folding and antecedent to the rest. The value of adjustment to internal structures in locating the river seems exaggerated. Even if essentially antecedent, the slight adjustments now found within the cluses might have taken place as the valleys were deepened. Moreover, it is highly probable that slight irregularities of structure, such as those to which Jenny ascribes the location of the existing cluses, would have been found wherever the cluses had been cut down across the anticlines. The frequent occurrence of cluses near the end of pitching anticlines is quite as suggestive of a course consequent upon initial folding and antecedent to later and greater folding as of dependence on slight local deformations.

SUB-OCEANIC CHANGES.

MILNE draws attention to sub-oceanic changes (London Geogr. Journ., X., 1897, 129–146) caused by slides of detritus down the marginal slopes of continental plateaus. Cables are there broken in consequence of their burial beneath such slides. The cause of the movement is ascribed to the accumulation of land-worn detritus. In Japan a large number of earthquakes come from the deep sea off the mouth of the Tonegawa, the largest of Japanese rivers, which crosses an alluvial plain in its lower course. Many other shocks on those islands have similar

origin. Where long continental slopes descend beneath the ocean at a steeper gradient than 1 in 35, slips and earthquakes of this kind may be expected. The western boundary of the Tuscarora deep, in the North Pacific, is a source of many earthquakes, among them the destructive disturbance of June 15, 1896. There Milne infers 'sudden sub-oceanic changes along the basal frontier of a continent, the magnitude of which it is difficult to estimate.' Certain 'unfelt earthquakes' recognized by the horizontal pendulum are recorded at widely separated stations, 'and it is fair to assume that in these instances the whole world has been shaken.' Their source cannot have been on any land, for then they must have been observed in the ordinary manner; they are therefore ascribed to submarine movements.

If the occurrence of sub-oceanic slides be verified, they afford a new argument for the permanence of continents and oceans; for nowhere do the sedimentary strata of the continents exhibit so confused a structure as must be thus produced along the slope and basal frontier of a continental mass.

W. M. DAVIS.

HARVARD UNIVERSITY.

CURRENT NOTES ON ANTHROPOLOGY.

STONE IMPLEMENTS FROM THE POTOMAC DISTRICT.

PROFESSOR WILLIAM H. HOLMES contributes to the Fifteenth Annual Report of the Bureau of Ethnology one of his excellent and beautiful papers, this one on the 'Stone Implements of the Potomac Chesapeake Tidewater Province,' 152 pages, with 104 full-page plates, and 36 figures in the text. The geographical and geological relations of the area are carefully explained, and the artefacts themselves are examined under the classification of flaked, battered or abraded, and incised or cut stone implements and utensils. The typical forms and characters are illustrated, the processes of manufacture are set forth, and the extensive quarries where the material was obtained are described.

The conclusion of the author, after years of patient research with reference to the antiquity of man's work in this region, may be given in his own words (p. 146): "The art remains preserved to our time indicate the prevalence of extremely simple conditions of life throughout the past, and exhibit no features at variance with those characterizing the historic occupancy." So that we shall have to go elsewhere to find 'paleolithic man.'

ETHNOGRAPHY OF THE CALCHAQUIS.

IN SCIENCE, May 7th, I referred to some interesting art remains discovered by Ambrosetti in the territory of the ancient Calchaquis. Ethnographers have been unable to identify these with any modern tribe (see 'The American Race,' p. 319). The latest effort is by Dr. Ten Kate. He availed himself of a series of skeletons in the Museo de La Plata, exhumed from old graves in Calchaqui territory. Some were deformed, and of the normal there were a number of types; but the characteristic features were extreme brachycephaly and a short stature. In both respects these ancient differed from the modern natives of the place. Looking around for similarities, Dr. Ten Kate found them among the Huarpes of the province of La Rioja, where skeletons with the same traits occur. He does not, however, identify them with the Huarpes (or Allentiacs), who are probably related to the Chaco tribes, but rather with the Araucanian stock, so far as one can judge from the synopsis of his conclusions in the *Centralblatt für Anthropologie*.

These analogies are not borne out by the linguistic evidence of the proper names in

Calchaqui territory, which rather points to Aymara or Kechua affinities as undoubtedly to the arts of this extinct population.

D. G. Brinton.

University of Pennsylvania.

NOTES ON INORGANIC CHEMISTRY.

MODERN ALCHEMY.

Dr. H. Carrington Bolton contributes to the last *Chemical News* an article entitled 'Recent Progress of Alchemy in America.' It is largely devoted to such details as are known of the claims of Dr. Stephen H. Emmens, of New York, and of Edward C. Brice, of Chicago, together with the Mint Report on the Brice process. Dr. Emmens is the inventor of the high explosive Emmensite, and the author of the 'Argentaurum Papers.' It is perhaps worth while to give a brief abstract of his claims, taken from his own publications. He states that he was led to his present study by the investigation in 1892, at the instance of Commodore Folger, of a specimen of rustless nickel steel, which it was proposed to use for torpedo netting. He says that he found in both nickel and iron, and subsequently in cobalt, 'a certain product which seemed to differ from anything recorded in the text books.' Inferring that, if such a substance were found common to the metals of the fourth series of Mendeléeff's group eight, similar results would be found in other groups, he began the study of gold and silver. 'By certain physical methods and by the aid of certain apparatus' he claims to have succeeded in bringing about an extremely minute subdivision of silver, and was 'surprised to find that the substance obtained differed so far from ordinary silver that it could no longer be regarded as the same elementary substance.' His alleged substance, which he calls *argentaurum*, symbol *Ar*, he considers to be the missing element between silver and gold in the second subdivision of Mendéeleff's group two. "Argentaurum can be aggregated into molecules having a density considerably superior to that of silver molecules, and, we think, identical with that of ordinary gold molecules. Whether we are right as to this or not, the condensed argentaurum presents the appearance and is endowed with the properties of ordinary metallic gold." Dr. Emmens estimates that 'one ounce of silver will produce three quarters of an ounce of gold' at a profit of at least three dollars an ounce. He operates on Mexican dollars, and has sold to the U. S. assay office six ingots of an alloy of silver and gold aggregating in value $954.80. Dr. Emmens remarks: "The gold-producing work in our argentaurum laboratory is a case of sheer mammon seeking; it is not being carried on for the sake of science, or in a proselytizing spirit; no disciples are desired and no believers are asked for. I have every confidence that the production of argentaurum gold will be brought up to 50,000 ounces monthly within a year." Should this result be attained, the problem of bimetallism will be happily solved.

Regarding the Brice process, three government experts worked for three weeks under Mr. Brice's direction, and officially report: "We have seen not the slightest evidence of any 'creation' or transmutation. On the contrary, the claimant failed in every instance to recover the entire amount of silver and gold known to be present in the materials. The claimant seems to have devised a variety of irrational and wasteful methods for recovering a portion of the silver and gold known to metallurgists as being present in many commercial metals, such as antimony and lead." Mr. Brice's application for a patent has been again rejected. Incidentally, the assay office investigation revealed that commercial antimony contains a very small percentage of gold, which is recovered by the Brice process.

APROPOS of argentaurum, Dr. Emmens and Newton W. Emmens publish in the *Chemical News* a short article on 'Migrant Matter,' describing an experiment in which a disc of pure lead connected with a disc of pure silver by a copper spiral was kept in a wide-mouthed bottle for twelve weeks. At the end of this period the lead disc on cupellation is said to have given a silver bead weighing 0.00003 gram. "It would appear from this experiment that what is commonly recognized as solid silver is, in part at least, a migrant mode of matter * * *. We use the term 'migrant matter' because the traveling particles to which we refer are (in common with odors generally) much more akin to Crookes's 'fourth form' than to gases."

J. L. H.

SCIENTIFIC NOTES AND NEWS.

THE INTERNATIONAL MEDICAL CONGRESS.

THE New York *Medical Record*, with great enterprise, secured by cable a report, extending to a number of pages, of the Twelfth International Medical Congress, which opened at Moscow on August 19th. After the Congress had been opened by the Grand Duke Sergius and welcomed by the Minister of Public Instruction, Hospodin Dylianov, Professor Sklifosovsky, the President of the Committee of Organization, made an address in which he dwelt especially on the relations of Russia to the rest of Europe in regard to medical and scientific work. He said that one great obstacle to medical progress was the want of a common medium of communication between the men of science of different nationalities. Nowhere was this almost fatal lack so fully realized as in Russia. The great mass of Russian medical literature was a sealed book to Western peoples, and few outside of the country had any conception of the enormous amount of scientific work that was being done there. Russians recognized that their language was too difficult ever to become universally known, and they were therefore the more keenly alive to the necessity of the adoption of some international speech. He would suggest the appointment of a committee to draft a preliminary agreement on the subject, which should be laid before the next congress for consideration. The General Secretary, Professor Roth, reported an attendance of 7,300 members in Moscow. Of this number more than 3,500 were from Russia, 800 from Germany, as many from Austria, 400 from France, 300 from Italy, 300 from England, 120 from the United States, 30 from Mexico, 10 from Japan and 4 from China. Prince Gallitzin, the Mayor of Moscow, then greeted the members of the Congress on behalf of the municipality and announced that the city of Moscow had decided to establish a prize of 5,000 francs, to be awarded at each international congress, to the person who in the interval since the preceding congress shall have done that medical work which shall be deemed of the greatest benefit to humanity. Addresses were then made by the national delegates, and the honorary presidents were announced as follows: Germany—Virchow, Leyden, Ziemssen and Waldeyer; Austria—Gussenbauer, Hlawa and Rudiger; Great Britain—Stokes, MacCormac and Simpson; United States—Senn and Thayer (Billings, it was announced, would have been one of this number had he not been absent); Spain—Robert; France—Lannelongue, Le Dentu, Grasset and Pinard; Italy—D'Antona, Bottini and Lombroso.

General addresses were given by Professor Virchow on the continuity of life; by Professor Lannelongue on the sclerogenic treatment of surgical tuberculosis; by Dr. T. Lauder Brunton on the relationship of physiology, pharmacology, pathology and practical medicine; by Professor von Krafft-Ebing on etiology of progressive general paralysis; by Dr. Senn on classification and surgical treatment of acute peritonitis; by Dr. Metchnikoff on the plague, and by Dr. Robert on the mutual relations of pathology and therapeutics.

The next international congress will be held at Paris in the summer of 1900, with Professor Lannelongue as President of the Committee of Organization.

THE SEAL FISHERIES.

DR. DAVID STARR JORDAN, Commissioner-in-chief of the fur seal investigations for the United

States, with Mr. George Clark, Secretary of the Committee, arrived at San Francisco on the 25th. Mr. Macoun, the Canadian Commissioner, had already left the Pribyloff Islands, and the British Commissioner, Professor Thompson, was about to leave on H. M. S. 'Amphion.' Mr. Lucas, of the United States Commission, remained behind for a week. President Jordan is reported in a press dispatch to have said that the breeding grounds showed a shrinkage of about 15 per cent. over the conditions of last season, and the hunting grounds a shrinkage of 33 per cent. This is about what was predicted by the American Commission last year. The primary cause of the shrinkage of females on the breeding grounds is the pelagic catch of last fall and this spring. To this is added the loss due to starvation of orphaned pups in 1894, which should this year have lived to give birth to their first pups. This starvation in 1894, affecting, as it did, in a like measure the male herd, is the cause of the diminution of the killable seals on the hunting grounds. The decline of the herd is everywhere more distinctly marked than it was last year, owing to the effects of the resumption of pelagic killing in Bering Sea after the *modus vivendi* of 1893. For 1898 the shrinkage will be still greater, through the destruction, in 1894, of unborn pups with females killed. Branding of young female seals, which will be begun after September 1st, will be carried on by Colonel Murray, chief agent on the islands, and E. F. Farmer, electrician. The skins of the branded cows which returned this year to the islands show clearly the permanency of the mark and its efficiency to render the skins unsalable, without injury to the animal or to the herd.

THE London *Times* has published a letter two and a-half columns in length from Mr. Herman Liebes, one of the lessees of the Prybiloff Islands seal rookeries, in which he presents the case for the United States in a temperate and reasonable manner, much more likely to prove convincing than the recently published letter of Secretary Sherman. The communication should make it clear to the ordinary reader that the decrease of the herd is not due to the loss of the seals killed on land, which are only young bachelors not serving harems, but to pelagic sealing, where at least 80 per cent. of those killed are females which are both pregnant and leave young to die of starvation on the islands. Since 1890 the annual catch of seals by the pelagic sealers amounts to an average of 88,916, as compared with an average of 15,770 from the islands.

THE MISSOURI GEOLOGICAL SURVEY.

THE following editorial comment appears in the *Journal of Geology:*

"The State of Missouri has recently had deep disgrace thrust upon it by the removal of the efficient director of the Geological Survey, and by the appointment of men to its care and conduct who possess, according to information that we deem trustworthy, not only no competency to perform their duties, but not even a plausible semblance of competency. These appointments have apparently no other motive than the conferring of personal or political favors. No causes of complaint, we are informed, were even alleged against the previous conduct of the Survey, or against the officials in charge of it. The scientific public has had ample demonstration of the vigor and energy with which the Survey has been prosecuted, the promptness with which its results have been published, and the adaptation of the work to the development of the economic as well as the scientific resources of the State. It appears, therefore, that the moneys appropriated by the State of Missouri for the laudable purpose of investigating and advertising its resources, and of informing its people concerning their own sources of material and intellectual wealth, are being virtually diverted from the purposes specifically indicated by the statutes of the State, and are being used for the personal and political interests of the Governor and his friends in the form of payment for worthless services. We are not sufficiently informed in the technicalities of law and the processes of the Courts to know how legal action in a case of this kind can be instituted and maintained, but if the appointees are as obviously incompetent as information indicates, they are simply consuming the funds of the State to no purpose save their own, and we think that an effort

should be made to procure a formal declaration by the Courts whether this is not a virtual embezzlement, and, if so, to secure the award of the proper punishment. If there is now no way of compelling a Governor to respect the laws of a State and the purposes of its statutes, a way should be provided."

GENERAL.

THE International Mathematical Congress, which closed its meetings at Zurich on August 11th, decided that its next meeting be held in Paris in 1900. A committee of five members was appointed to report on mathematical progress in different countries and to promote the establishment of mathematical departments in academic and other libraries. A resolution was passed favoring the institution of a central correspondence bureau in order to maintain closer relations between teachers and students of the higher mathematics in different countries.

THE attendance at the recent meeting of the French Association for the Advancement of Science at Saint-Etienne was smaller this year than usual, amounting to scarcely 400.

PRINCE LUIGI, of Savoy, and his party succeeded in reaching the summit of Mt. St. Elias at noon on July 31st. The altitude was determined to be 18,120 feet; no indications of volcanic action were found. It is said that the expedition under Mr. Bryant reached an altitude of 14,500 feet—the height reached by Professor Russell in 1891—and was compelled to return owing to illness.

IT will be remembered that Congress appropriated $5,000 for a continuation of the survey of the gold resources of Alaska, but the bill containing this item was passed too late to make it available this year. The expedition under Mr. J. E. Spurr will go next year, and it is proposed to ask for an increase of the appropriation to $25,000 in order that a complete survey may be made and a geological map of the region prepared.

PROFESSOR CORFIELD has been elected an honorary member of the Royal Society of Public Health of Belgium, of which he has been a corresponding member for some years.

PROFESSOR BELAJEFF has been appointed director of the Botanical Gardens in Warsaw.

IN addition to the degrees conferred on Lord Kelvin, Lord Lister and Sir John Evans by the two universities located at Toronto, McGill University will this week confer on them the doctorate of laws. The convocation of McGill University will be held in recognition of the meeting of the British Medical Association and will confer the degree on Dr. L. L. Barnes, President of the Association, and probably on others in attendance.

A SPECIAL number of the *Rendiconti della R. Accademia dei Lincei* announces, says *Nature*, the award of the following prizes, besides others for essays of a literary character: The royal prize for physics to Professor Adolfo Bartoli, of Padua, for his two monographs on the specific heat of water between the temperatures of 0° and 35°, and on the heat of the sun and for other investigations. For the ministerial prize for physical and chemical science eight competitors entered, and the judges have awarded a prize of 1,000 lire to Professor Carlo Bonacini, of Modena, for his essays on orthochromatic and color photography, and on the reflection and other properties of Röntgen rays; also awards of 250 lire each to Professor Carlo Cattaneo, of Turin, for his notes on the conductivity of electrolytes and on the velocity of ions, and to Professor Pietro Bartolotti for chemical investigations relating to the compound Rottlerine and other derivatives.

WE learn, with much regret, of the death of Professor J. E. Humphrey, which occurred about the middle of August at Jamaica, where he had charge of a biological expedition from the Johns Hopkins University. Professor Humphrey was a graduate of Harvard University and was a young man, having been promoted only last year to an associate professorship of botany at Johns Hopkins University. He had made important contributions to our knowledge of the algæ and in other directions, and was building up a strong department of botany in the Johns Hopkins University.

WE regret also to announce the death, at the early age of 23 years, of Mr. Joseph G. C. Cot-

tier, fellow of Columbia University. Mr. Cottier had been traveling in France since the first of July, and died from typhoid fever at Paris on the 17th of August. He was a student of exceptional ability in the mathematico-physical sciences and gave promise of a brilliant career.

THE deaths are also announced of Mr. Albert Marth, fellow of the Royal Astronomical Society and the author of many valuable contributions to astronomy, at the age of 69, and of Mr. S. E. Peal, of Assam, a writer on astronomical topics.

A CABLEGRAM to the daily press reports that the British steamer Windward, which left England on June 10th last, for Franz Josef Land, to bring back from the Arctic regions the members of the Jackson-Harmsworth expedition, who have spent three winters near Cape Flora, passed Aberdeen on Aug. 28th on her return trip and signalled that all were well on board. During the present summer the expedition was to make an attempt to reach the highest point north through an opening in Queen Victoria's Sea, the open water discovered by Jackson.

REUTER'S agency announces that M. Nossiloff, the explorer, has arrived at Tiumen from the Kara Sea. He has made important discoveries connected with intercourse between Siberia and Europe, especially a direct waterway which is considerably shorter than the old one and is not affected by the sea ice. M. Nossiloff also announces that he has investigated the hitherto unexplored Yalmal peninsula.

PROFESSOR W. W. CAMPBELL will have charge of the expedition of the Lick Observatory to India to observe the total eclipse of the sun on January 21st of next year. As we have already stated, this expedition has been made possible through the generosity of the late Col. C. F. Crocker.

THE Boston *Transcript* states that Mr. Edward Kemeys, the animal sculptor, will probably receive a commission for the decoration of the zoological park in Washington. It is proposed to decorate the park with bronze figures of Indians and wild animals, of life size or a little larger, to preserve the types of animal life which are rapidly vanishing. The Indians will be represented in some pieces as alone and in some pieces as riding or hunting the animals. The animals will sometimes be separate and sometimes grouped together, as in the case of a leopard killing a deer.

IT is reported that as soon as the new library of the Moscow University is completed, the well-known savant, Dr. Luginin, has promised to make it a present of his fine collection of books on chemistry, embracing 12,000 volumes, besides adding a sum of money for purchasing new books on that subject.

THE annual meeting of the corporation of the Marine Biological Laboratory at Woods Holl was held at that place on August 26th. The chief business was the election of a board of trustees and the enactment of some changes in the by-laws. Seventy members were in attendance. A resolution was adopted ratifying certain changes in the constitution proposed at a special meeting in Boston on August 16th. Among the more important amendments adopted were those providing that the number of trustees be increased from twenty to twenty-four, and that ex-officio members be made members of the board of trustees. Trustees were elected as follows: Clerk, Professor H. C. Bumpus, Brown University; Dr. E. G. Gardiner, Professor Clark, Williams College; Dr. J. P. McMurrich and L. L. Nunn, University of Michigan; Professor H. F. Osborn and Professor E. B. Wilson, Columbia; Professor William Libbey and W. B. Scott, Princeton; Professor W. C. Sedgwick, Massachusetts Institute of Technology; Professor Sydney W. Smith, Yale; Dr. William Trelease, Missouri Botanical Gardens; Professor W. P. Wilson, Philadelphia Museum; Professor B. Ramsey Wright, University of Toronto; Professor J. M. McFarlane and E. G. Conklin, University of Pennsylvania; Professor E. P. Mall, Johns Hopkins University; Professor E. L. Mark, Harvard; Samuel P. Scudder, Cambridge; Lawrence Minot, Boston; Professor T. H. Morgan, Bryn Mawr; C. G. Kidder, New York; Professor M. M. Metcalf, the Woman's College of Baltimore; Professor William Patten, Dartmouth; Professor D. P. Penhallow, McGill University.

Nature learns from the *Brisbane Courier* that Hon. R. Barr-Smith, of Torrens Park, South Australia, has offered to subscribe the amount

necessary for the establishment of a tentative meteorological station at the summit of Mount Kosciusko, in the southeastern extremity of Australia, at a height of about 8,000 feet. Mr. Wragge hopes that the comparison of the results with those obtained from low-level stations will insure a permanent observatory being established in the interest of Australasia.

M. JANSSEN was expected, says the Paris correspondent of the London *Times*, to leave Paris on August 21st to inspect his Mont Blanc observatory. The scientific caravan will start from Chamonix at the beginning of September to take meteorological readings at an altitude of 14,000 feet, the highest station in the world, and to study solar radiation. M. Perrotin, director of the Nice observatory, will, it is said, join the party in order to study the morning elongation of Venus and to ascertain the period of the planet's rotation. Foreign men of science wishing to make the ascent will enjoy the hospitality of the establishment.

AT the monthly general meeting of the Zoological Society of London, held on August 19th, at their offices in Hanover-square, it was stated that the number of additions to the Society's menagerie during the month of July had been 102, among which special attention was called to a young pair of Babirussas (*Babirussa alfurus*), from Celebes, presented by the Duke of Bedford; to an example of the thick-billed penguin (*Eudyptes pachyrhynchus*), from Stewart Island, New Zealand, deposited by the Hon. Walter Rothschild; and also to a very large example of Daudin's tortoise (*Testudo daudini*), deposited by the same gentleman. The President delivered the silver medal of the Society to Mr. Alexander Whyte, F.Z.S., in acknowledgment of the valuable services he had rendered to zoological science by the large collections he had made in every branch of natural history in British Central Africa, while acting as chief scientific officer to Sir Harry Johnston.

THE second International Conference on Railway and Marine Hygiene will, as we have already announced, meet at Brussels on September 6th, 7th and 8th. Several governments and societies have appointed official delegates, and numerous papers have been promised. There will be three sections: (1) Organization of the Medical Services; (2) Guarantees of Personal Fitness; (3) Hygienic Rules and Regulations. The officials and medical officers of foreign railway companies can obtain free passes upon the Belgian state railways. Dr. J. De Lantsheere, 56, Rue de l'Association, Brussels, will supply further information.

THE British Pharmaceutical Conference, which has recently concluded its sessions at Glasgow, will next year meet at Belfast, Dr. Symes, of Liverpool, being reelected President. One of the more interesting papers presented to the Conference was by Professor Atkinson, of London, showing that the people of Great Britain expend annually £3,000,000 on patent medicine. In 1872 the annual revenue from patent medicine stamps was £82,000; ten years later that amount was doubled, and in 1892 the tax produced £240,000. He thought that the government should exercise more control over the sale of these secret nostrums.

THE *Progrès médicale* publishes a somewhat extended account of the International Bibliographical Conference which met in Brussels on August 2d, 3d and 4th. A special discussion took place on the Dewey decimal system of classification, which was opposed by M. Funck-Brentano and defended by M. Baudouin and others. Dr. Haviland Field announced that the delayed *Concilium bibliographicum* for zoology would soon begin the issue of cards. M. Baudouin stated that within three years nearly 200,000 bibliographical cards with analyses, relating especially to the biological sciences, had been prepared for the Paris Institute of Bibliography.

MR. MOSES P. HANDY, special commissioner to the Paris exhibition of 1900, has gone to Paris to make arrangements for the space and location of exhibits from the United States. He is reported to have said before leaving that he thought 500,000 square feet of space would be required by American exhibitors, or more than four times as much as was in the exposition in 1889. The exhibits in electrical and railway appliances are expected to be especially complete. A thorough educational exhibit will also be prepared.

THE exhibition now open at Brussels contains a feature of interest to anthropologists. Two hundred natives of the Congo tribes have been brought from Africa and are exhibited in a replica of a Congolese village. The greater number are Batatelas, but other tribes are represented, including two of the forest dwarfs described by Stanley.

THE first number of the *Zoological Bulletin*, edited by Professors C. O. Whitman and W. M. Wheeler, of the University of Chicago, has now been published by Ginn & Co. The title page gives the names of seventy-three American students of the zoological sciences who have given their cooperation. This number contains articles by Edward Phelps Allis, Jr., on 'The Morphology of the Petrosal Bone and of the Sphenoidal Region of the Skull of Amia Calva;' by Charles W. Hargitt on 'Recent Experiments on Regeneration;' by Charles Lawrence Bristol on 'The Metamerism of Nephelis,' and by G. Baur 'On the Question of Intercalation of Vertebræ.' The number extends to fifty-three pages and it is expected to publish at least six numbers of this size yearly. The *Bulletin* has the same form and size of page as the *Journal of Morphology*, and will apparently differ from that journal only in that the articles will be as a rule shorter and not illustrated.

A QUARTERLY botanical journal, entitled *Bollettino del Reale Orto Botanico di Palermo*, will hereafter be published, under the editorship of Professor Borzi.

HENRY HOLT & CO. announce for early publication 'The Elements of Comparative Zoology,' by Professor J. Sterling Kingsley, of Tufts College; 'Laboratory Directions in General Biology,' by Harriet Randolph, instructor in Bryn Mawr College; an 'Outline Introductory to Kant's Critique of Pure Reason,' by Professor R. M. Wenley, of the University of Michigan, and a new and much enlarged edition of Hall and Bergen's 'Text-Book of Physics.'

WE are glad to note the full reports in the American daily press of the recent meeting of the British Association at Toronto, even though some of these papers did not mention the meeting at Detroit. We hope that all the papers will remember next year that there is an American Association for the Advancement of Science.

THE fourth International Scientific Congress of Catholics opened its sessions at Freiburg, Switzerland, on the 15th of August. Mgr. Dernaz, Bishop of Lausanne and Geneva, was the president of honor, and Baron Hertling, professor of philosophy in the University of Munich, was made president of the Congress. The Congress met in ten sections, and there were 500 members in attendance.

THE *Engineering News* Publishing Company have republished three articles on the floods of the Mississippi River contributed to the journal in the course of the past two years by Mr. William Starling, Chief Engineer of the Lower Yazoo Levee District. The articles describe the levees of the Mississippi river and the floods, especially that of 1897, and should prove useful in disseminating knowledge of the cause of the floods and the proper means to be used in combatting their ravages.

THE *Electrical World* reports a paper by M. Pierard describing an application of the phonograph in Spain. It appears that the telephone is there used in place of the telegraph, and it was found that the speed of transmission was greatly limited by the operators at the receiving end, who could not transcribe the messages sufficiently rapidly; at present the receiving operator repeats the message into a phonograph, from which it is then transcribed; this repetition is also heard at the transmitting end and therefore serves as a control for the correctness of the received message; the speed is 86 words per minute, which means double this amount if the return message is included. It is thought that this is the first time the phonograph has been put to this use.

A BRITISH Parliamentary paper has been issued given a report of the efforts to check the locusts in Cyprus during the past year. The High Commissioner, Sir W. J. Sendall, states that the system of purchasing eggs and living locusts, which has now been pursued for four years, has appreciably diminished the number of locusts.

THE first quarterly report, says the New

York *Medical Record*, since the appointment of medical inspectors of schools in this city has been made by Dr. Blauvelt, the chief medical inspector. The report includes a table showing the different kinds of diseases for which children were excluded from the schools. Parasitic diseases of the head appear to have been the most prevalent, 2,627 cases having been discovered among the children examined. Contagious diseases of the eye come next on the list, over 700 cases being reported. Skin diseases claimed 175 victims, and diphtheria 91. Measles was responsible for the exclusion from school of 51 children, and 20 cases of genuine scarlet fever were discovered. Croup was of comparatively rare occurrence, but 26 scholars were compelled to forego school attendance for a short time on account of whooping-cough. The report gives the number suffering from mumps as 117, and from chicken-pox as 93.

IT appears from an article in *Machinery* that the Stockholm Exhibition, which has attracted but little attention in the daily press, is of considerable scientific interest. The exhibition grounds cover an area of about 220,000 square yards—a space which is about doubled if the mountain plateau, which is given up to a realistic reproduction of Swedish country life from the Laplander's hut to the thriving farmer's homestead, with all the fauna and flora of the country, is added to it. The most imposing building in the grounds is the Industrial Hall, which is said to be the largest wooden structure in the world, a forest of about 34,000 trees having been used up in its elevation. The exhibits in this building are selections from the ordinary products of the industries of Sweden, Norway, Denmark and Russo-Finland. Not far from it is the Machinery Hall, and in close contiguity are mining exhibits, forest exhibits, electric works and numerous private pavilions illustrative of the industrial works of the country.

PROFESSOR RAPHAEL MELDOLA writes to the London *Times* regarding the case of death from an inflammable hair wash which has excited much discussion in Great Britain. "It is, of course, well known to those who are in the habit of dealing with low boiling-point liquids giving off inflammable vapors, such, for example, as ether, that the presence of a naked flame, even at a distance of many yards, is attended with great danger. But in the present case Professor Meldola corroborates by personal experience the contention of Lord Kelvin that an electric spark, although in itself feeble, is quite sufficient to ignite an explosive mixture of hydrocarbon vapor and air. Some years ago he was consulted by a manufacturer who had an extensive business in what is called the 'dry cleaning' of wearing apparel. In this process the goods, previously well dried, are immersed in a vessel of benzine, in which they are kept in motion by means of mechanical stirrers. In spite of every precaution to insure the absence of naked flames in the building in which the operation was conducted, fires were constantly occurring, and at one time the process threatened to become a failure on account of this apparently spontaneous inflammability of the hydrocarbon vapor. It was only when the possibility of the generation of electricity and the passage of sparks were suggested that precautions were taken to exclude air from the upper portions of the vessels and that the process became practicable. The experience afforded by this process points most conclusively to the correctness of the electric spark theory of the ignition, and on this ground alone the use of such inflammable hydrocarbons for cleaning the hair—apart from the very doubtful question of their efficacy—should be absolutely condemned."

THE Executive Committee of the International Medical Congress has followed the example of its predecessors at earlier gatherings in publishing descriptions of the medical institutions of the country. The *British Medical Journal* gives an account of one of these, containing a full description of the medical societies of Russia. The oldest Russian medical society was founded in Moscow in the year in 1804, and is still in existence. It is attached to the University of Moscow, and is known as the Physico-Medical Society. A year later the Vilna Medical Society was instituted; it also still exists, but owing to the chequered career of the University of Vilna (which was removed to Kief after the Polish rising of 1833) this society has lost something of its former prestige.

The institution of these two societies was almost coincident with the opening of the three Universities of Kharkof, Kazan and Vilna by the Emperor Alexander I. Before his reign there had been but one university in the Empire, that of Moscow. Towards the close of his reign, in 1819, still another university, that of St. Petersburg, was opened; and in the same year the Society of German Practitioners in St. Petersburg was formed. This was followed a year later by the formation of a similar society in Warsaw. A gap of over thirty years, corresponding to the reign of the Emperor Nicholas I., succeeded, during which but one medical society was instituted in Russia. This was the Society of Russian Practitioners in St. Petersburg. From the date of the accession of Alexander II. the number of these societies has rapidly increased. There are now no fewer than 120 in the whole of the Empire, the total capital of which amounts to over $600,000. The majority of Russian medical societies have libraries attached to them; while some have museums, laboratories and even free dispensaries.

UNIVERSITY AND EDUCATIONAL NEWS.

It is announced that M. Eulogius Georgieff, the founder of the Sofia University, who died recently, bequeathed 20,000,000 fr. to the Bulgarian government for public purposes, including 6,000,000 fr. for a technical school to be established at Sofia.

Mr. S. B. Brownell has presented Barnard College, New York, with a building for a dormitory to cost $150,000, which will be erected at once.

The endowment left by Patrick B. O'Brien, of New Orleans, for establishing chairs of chemistry, of physics and of Roman law in the Catholic University of America will be available this autumn. The Rev. Father W. J. Kirby has been appointed professor of sociology.

A committee of the Trustees of the University of Missouri has been appointed to investigate the political views of Professor C. H. Hicks, since 1892 professor of political economy in the University. The report of the committee will probably result in the removal of Professor Hicks, and it is said that Dr. W. G. Brown, professor of chemistry, is likely to share the same fate.

The Council of the University of Lyons has devoted the sum of 42,000 fr. to complete the construction of the laboratory of biology of Tamaris, near Toulon, and it is expected that a sum will be appropriated to the support of the laboratory.

The School of Applied Chemistry, founded last year under the auspices of the faculty of science of the University of Paris, has celebrated its first anniversary. M. Darboux presided, and an address was made by M. Friedel, director of the laboratory.

The University of Berlin offers holiday courses for teachers of science in the higher schools from the 29th of September to the 9th of October of the present year. Lectures will be given by Drs. van't Hoff, Dames and other leading professors, and excursions and visits of scientific interest have been arranged.

Dr. Kihlman has been appointed associate professor of botany in the University at Helsingfors. Dr. Theodor Petersen and Dr. Josef Epstein, of the Physical Institute of Vienna, have been appointed to professorships. Mr. Stanley Dunkerley, of the department of applied mechanics, Cambridge, has been appointed professor of applied mechanics at the Royal Naval College, Greenwich.

DISCUSSION AND CORRESPONDENCE.

'THE PRESENT EVOLUTION OF MAN.'

In Science of July 2d, pp. 33–35, Professor T. D. A. Cockerell published a kind and appreciative review of my book 'The Present Evolution of Man,' for which I am more than grateful. He disagreed with me, however, as to several matters, and as on these precisely I have failed to carry conviction to many critics I am forced to believe either that I am wrong or that my explanations have not as yet been adequate nor sufficiently clear. I would fain hope the latter, and therefore am glad to seize the opportunity Professor Cockerell's review affords of making some reply in the pages of Science.

Professor Cockerell does not entirely agree with my theory of retrogression. Put very

briefly, that theory is as follows: It is known that the ontogeny recapitulates the phylogeny, though in a rapid, blurred and indistinct way.* In other words, almost every individual recapitulates the traits of all his ancestors, beginning with those of the first (as, for convenience, we may call the unicellular organism) and ending with those of the last. I use the word 'almost' because, though the earlier stages must, of course, be recapitulated, for otherwise the individual would not develop, yet sometimes (indeed often) the very last steps made in the evolution of the race fail to be reproduced by the individual in his development. He then resembles some remote ancestor more than he does his parents, and presents, in fact, an example of what is known as atavism. When the evolution of the race has been rapid, reversion to any given ancestor results, of course, in a much greater and more observable degree of retrogression than when the evolution has been slower, and therefore, while offspring of race horses often exhibit far-reaching reversion towards the ordinary horse, and while the seedlings of various garden plants (apple, peach, rose, etc.), which have been evolved under excessively stringent selection, generally revert to the ancestral type, the offspring of wild plants and animals generally 'breed true.'

An individual may vary from his parent in two ways—towards the ancestry when some of the last steps made in the phylogeny are omitted in his ontogeny, or away from it, the former variation being atavistic, the latter evolutionary; and so far as we are aware the chances of his doing the one or the other are equal. But while every variation towards the ancestry, that is, every failure to repeat in the ontogeny the last steps made in the phylogeny, produce atavistic retrogression, every variation from it need not be an extension of the previous evolution; it may constitute a reversal of it (as will be seen presently) or be in some other different direction, whence it follows that in the absence of selection a species must always undergo retrogression.

Up to this point, or nearly up to it (for as to this I am not very clear), my critic and I seem to be in agreement; but beyond this we differ, for while he thinks that such atavism can result in very limited retrogression only, I am of the opinion that in the entire and continued absence of selection it must result in absolutely unlimited retrogression. Before dealing with his objections I ought to say that in my book I quite reject the Lamarckian doctrine of the transmissibility of acquired traits, and since Professor Cockerell is good enough to say that he regards the arguments there used as conclusive, the matter is not in dispute between us. Moreover, he seems to agree with me in attributing evolution to the accumulation of small 'normal' variations, not to the accumulation of great and abnormal variations, for the reason that the latter tend to be swamped, owing to their infrequency. I should also make it clear that by atavism I do not necessarily mean atavism of the whole organism. Such wholesale reversion to the ancestral type must be extremely rare in the case of all species slowly evolved under the ordinary conditions of nature; it occurs only, so far as I am aware, in such species as have evolved under very stringent artificial selection. Under natural conditions an individual may exhibit evolution in some particulars and atavistic retrogression in others, but these latter, owing to the slowness of the antecedent evolution, must generally be minute in amount, and, therefore, when associated with evolutionary changes, unrecognizable in the individual, though recognizable after the lapse of generations in a line of individuals. Thus, while selection may result in evolution in the legs of a bird to which flight has become useless, atavistic reversion in the absence of selection as regards them would result in the retrogression of its wings. But here again, the wings, being complex organs, would not equally retrogress in all their parts, and, therefore, would never very closely approximate to the ancestral type.

Professor Cockerell says: "I cannot see, with Mr. Reid, that there would be unlimited atavism, because when the atavistic changes had proceeded from B to A *the B features would become ancestral* and a new atavism from A to B would appear." (The italics are mine.) It is

*I have given reasons why the recapitulation is blurred and indistinct, but these need not detain us here.

this sentence which leads me to suppose that I have not adequately explained myself, for clearly Professor Cockerell does not understand my theory as I understand it. Supposing, as regards any character, A B C D represent a line of individuals, then, if D reverts to B, that is, if D varies from C so that in his ontogeny he repeats the life history of the race only up to the point reached by B, omitting the additional characteristics of C, my contention is that C disappears altogether from the series, which, from the point of view of heredity, becomes A B D and, therefore, if the characters of C ever reappear in E, or any subsequent member of the series, they must do so (as they did in C's case) as a result of evolution (variation away from the ancestry), not as a result of retrogression, of atavism (*i. e.*, variation towards the ancestry).* To test the truth of the theory we must consider a species in which evolution has been very rapid, as in that case atavistic changes can easily be observed with comparative ease; for example, any one of the various garden plants which are propagated by means of slips, it being probable that but a few seminal generations intervene between these widely divergent types and the ancestral wild varieties. The seeds of such a cultivated plant usually give origin to plants, which, in the great majority of instances, resemble much more nearly the wild than the cultivated variety. Now, if Professor Cockerell is right, the continual seminal propagation of such reverted plants should often result in a 'reversion' to the cultivated type; whereas, if I am right, this 'reversion' should never occur, since (for purposes of heredity) the cultivated variety has been eliminated from the ancestral line. Such reappearance would purport, therefore, not reversion, but a fresh evolution, which evolution would be too great to be accomplished in a single generation. So also the race horse, in the absence of selection, would revert to the ordinary horse, after which a race horse should be as rare among the progeny as it is at present among the progeny of ordinary horses.

It follows as a logical conclusion from the foregoing that cessation of selection, as regards any structure or character, during unlimited time must result in unlimited atavism. First recent ancestors would be approximated to, then ancestors more remote, and lastly that remote ancestral condition when the structure or character did not exist. It would then disappear as utterly as have done, from this cause, the limbs of serpents. In fact, atavism effects the same result as disuse is said to do by Neo-Lamarckians, and, unless we accept the Lamarckian doctrine, I confess I am unable to perceive any other probable reason for retrogression.

Professor Cockerell further writes: "The germ, it must be supposed, contains units representing many phases of existence, while others are new. When one of the latter develop we say the variation is a progressive one; when the former develop we call the result atavism. It is reasonable to suppose that environmental and germinal selection are the factors which determine which of its possible developments the germ shall undergo." Here, again, is proof that I have not made myself clear on this point to Professor Cockerell. He has been kind enough to speak with some approval of my theory, but he has not perceived that it is absolutely incompatible with the theory of germinal selection. Surely in the present state of our knowledge it is too much to affirm that the germ *must* contains *units* 'representing phases of existence.' During the ontogeny many phases of existence are represented in fleeting but orderly succession, but again, surely it is not essential to suppose that the development of each is due to one or more *units;* and, moreover, does not the regular recapitulation of the phylogeny during the ontogeny negative the idea that any 'units' are 'held over?'

I must be brief in my comments on the rest of Professor Cockerell's review. He says that in considering man's present evolution I have neglected to take into account what Mr. B. Kidd has called 'social efficiency.' If he turns to my work (pp. 178–200) he will find that I have not altogether neglected the matter. Briefly, I have attributed differences in social

*For the sake of simplicity I do not here consider such a case as when D repeats the life history up to C and *then* reverts *back* to B, but it is dealt with to some extent in my book, and the process is of importance when considering the phenomena connected with 'reversed selection.'

efficiency to differences in acquired mental characters, that is, to differences in education. Mr. Kidd attributes social efficiency, which he derives from the greater or lesser development of the altruistic feeling, to natural selection; but natural selection implies elimination of the unfittest, and he has failed to record a single death as due to the absence of this feeling in him who perished and the presence of it in him who survived. If it be maintained, as is sometimes done, that 'social efficiency' has been evolved, not through selection of individuals, but through selection of communities, then I can only say that such a contention appears to me to involve a complete misconception. Evolution can result only from the selection of individuals, never from the selection of communities; except, indeed, in the case of such communities (*e. g.*, ants, bees, terits) as are the progeny of a single pair of individuals, when the parent individual is selected in the person of the progeny. Supposing a superior community (of men, for instance), without individual selection within itself, causes the elimination of an inferior community, then, as a consequence, the former spreads, but does undergo evolution. Its superiority (if inherent, and not acquired) arises from an antecedent selection of individuals within itself. Professor Cockerell says that sanitary arrangements 'do select the citizens of one town, state or country as against others, or those of the rural districts as against the towns.' But surely sanitary arrangements do not come under the head of human evolution, but under what has been called 'Evolution in the Environment.' The knowledge of it is acquired. If New York now values sanitation more than it did fifty years ago, or more than Baltimore does at the present time, is this not due, beyond doubt, to a difference in education (*i. e.*, acquired mental characters), not to an inborn intellectual difference?

I now come to the last of Professor Cockerell's objections and I wish exceedingly I had more space to deal with it. I have shown that certain powerful narcotics (*e. g.*, alcohol and opium) are great causes of elimination; that races (*e. g.*, Greeks, Italians, South Frenchmen, Spaniards, Portuguese) which have long possessed a cheap and abundant supply of alcohol, for instance, are the least prone to excessive indulgence of all races on earth; that other races, (*e. g.*, Anglo-Saxons, Scandinavians, Russians, etc.), which have had a less extended experience are more prone to intemperance; that yet other races (*e. g.*, savages of all kinds, whether inhabiting the frigid, the temperate or the torrid zones), who have had little or no experience of alcohol, crave for that narcotic so intensely that, in the presence of an abundant supply and the absence of prohibitory laws, they perish of excessive indulgence; and have argued, firstly, that the craving for narcotic indulgence was inborn in man as a by product of mental evolution;* secondly, that the Italian, for instance, is more temperate than the American Indian, as a result of natural selection, *i. e.*, alcoholic selection; and thirdly, that to render a race more temperate we must eliminate, not drink, but the excessive drinker, for compulsory temperance must lead to the survival of the unfit and consequent retrogression of the race to the ancestral type, when the craving was stronger than it now is in a race which has undergone alcoholic selection.

Had I proved my facts and used this line of argument as regards any physical structure I think all the world would have agreed with me; but, because I dealt with the burning question of intemperance, I have met with numerous objectors ranging from a clerical gentleman, who found distinct points of resemblance between Satan and me, and argued that, *therefore*, my theory 'must be a lie,' through Professor Ray Lankester, who, while admitting the truth of the theory, apparently thought that a tendency to get excessively drunk might be necessarily correlated to extremely valuable quali-

* Just as the paresis which accompanies excessive fear is inborn, *e. g.*, in frog when attacked by snake, as a by product, a correlated variation, of the life saving faculty of fear. Of course, I do not mean that an American Indian, for instance, who has never tasted alcohol, craves knowingly for it; I merely mean that he has an inborn love for such feelings as are induced by deep indulgence, just as he has an inborn love for such feelings as may be induced by eating a delicious food. Till he tastes a peach, for instance, of course he does not knowingly crave for it, yet nevertheless the love for the peach is born and in like manner is that for alcohol.

ties, up to Mr. Alfred Russell Wallace and to Professor Cockerell, who thinks "that the human race has no natural craving for alcohol at all, but it has a craving for excitement and other states of mind which may be induced artificially, and that when the natural exercise of highly valuable faculties is denied, as is so often the case in our present civilization, artificial means, often highly injurious, will be resorted to." But it is the absolute savage who is most prone of all races on earth to excessive indulgence, and surely, living, as he does, in a 'state of nature,' the natural exercise of 'highly valuable faculties' is not denied to him. Moreover, if Professor Cockerell is right, what is the difference between the civilization of the South and North of Europe, which permits in the former case the natural exercise of valuable faculties, but forbids them in the latter, for the north Europeans are much more drunken than the south Europeans.

Let the reader think awhile. Why does he not get drunk? Is it because he constantly resists the craving, or because the craving does not exist in him? I think he will say, 'the latter.' But has he no acquaintance, reared and living under much the same conditions, who drinks, to excess, though all his interests call him to abstain? I think he is sure to have such an acquaintance. Now, in this respect nations like the Italians or the Spaniards are mainly composed of individuals like my reader, while nations like the American Indians or the native Australians are mainly composed of individuals like his unfortunate acquaintance.

Here is a significant fact: old records seem to prove that the classic races were anciently much more intemperate than at the present time. For instance, the temperance question was formerly a burning one in Greece, where unhappy Helots were made to furnish 'awful examples' to the aristocratic youth. Here is another: the deadly narcotic opium has been in use for some hundreds of years in India, and never or very rarely does a native of that country take it to excess; it has been in use for about two hundred years in China, and most of the Chinese are temperate, though some take it to excess; it has been recently introduced into Burmah, and, practically speaking, all Burmans take it to such excess that they perish of it, and, therefore, in their own country the English have forbidden the use of opium to Burmans alone, while permitting it to all other peoples, just as in Canada alcohol is forbidden to the aborigines alone. Here is a third: tobacco causes little or no elimination, and, therefore, the craving for it is as strong in races that have longest used it as among races to which its use is comparatively strange.

G. Archdall Reid.

Louthsea, England.

AMPHIBIA OR BATRACHIA.

Professor Burt G. Wilder has made some remarks in the last number of Science (August 20, 1897) about the French word *Batraciens*. He says: "Dr. Baur shows that the French word *Batraciens* was applied to the frogs, toads and salamanders by Brogniart in 1799, and that the Latin forms *Batrachii* and *Batrachia* were not introduced until 1804 and 1807, by Latreille and Gravenhost. But does not Dr. Baur lay undue stress upon the distinction between the French and the Latin form? *Batraciens* is not (like *crapaud*, etc.) a vernacular word; it is the French form, or, galloparonym (!), of the Latin *Batrachia*, and the employment of the former would seem to constructively sanction the use of the latter." Professor Wilder 'as a teacher of zoology, but without claim to expert authority upon taxonomic points,' seems to be absolutely ignorant of the fundamental rule in nomenclature (published in all Codes of Nomenclature), that all vernacular names, of genera, families, orders, classes, even if formed from a classical root, are never accepted. Such vernacular names have especially been used in France by Cuvier, Lesson, de Blainville and notably other French writers of the early part of the present century. Such names have in many cases been later adopted into the science under a proper classical form, and should take date only from this later introduction. I should like to recommend to Professor Wilder the study of 'The Code of Nomenclature adopted by the American Ornithologists' Union, 1892.' This code is followed by all American naturalists. The case of *hippocampus* referred to by Professor

Wilder has nothing to do with zoological nomenclature.

G. BAUR.

THE SOURCE OF METENCEPHALON AND OTHER LATIN NAMES FOR THE SEGMENTS OF THE BRAIN.

TO THE EDITOR OF SCIENCE: In my paper, 'The definitive encephalic segments and their designations,' read before the Association of American Anatomists last May, were offered objections to the action of the Anatomische Gesellschaft* in designating the region between the cerebellum and the myel (spinal cord), not by *metencephalon*, as in the last three editions of Quain's 'Anatomy,' but by *myelencephalon*. This last was proposed by Owen for the entire neuron (central nervous system) in 1866 or earlier, and, so far as I know, its application to a single segment was made by Huxley in 1871. Before printing the paper above named, I desire to ascertain when and by whom that region of the brain was first called metencephalon; incidentally, also, the source and date of the other words, *prosencephalon*, *diencephalon*, *thalamencephalon*, *mesencephalon* and *epencephalon*, that have been offered as equivalents for von Baer's names, *Vorderhirn*, *Zwischenhirn*, *Mittelhirn*, *Hinterhirn* and *Nachhirn*. Information through your columns or directly will be very welcome.

BURT G. WILDER.

ITHACA, N. Y.

ANDRÉE'S NORTH POLE BALLOON VOYAGE.

IN SCIENCE for August 20th, p. 291, occurs a copy of a telegram purporting to come from Dr. Nils Ekholm regarding the Andrée balloon which is attracting so much attention. One serious error in the transcription should be corrected. It is stated that the balloon at the start rose to a height of 15,000 to 25,000 ft. The original may have been 150 to 250 metres (490 to 820 ft.), but could not possibly have been as given. To ascend 25,000 ft., over 3 tons of ballast would have to be thrown out, and this, of course, is not thinkable. At 25,000 ft. two-thirds of the gas would have been lost and the voyagers would have been in great danger of freezing to death.

It is a little difficult to understand Dr. Ekholm's figures. Admitting that 1,800 cubic feet of gas leaked out each 24 hours, entailing a loss in buoyancy of 123 pounds, as he gives it, there should still be enough gas for over 70 days, instead of 22 to 24 days, as given. A leakage of 1,800 cubic feet would be about 1%, which is not excessive, though about 2 times as much as was expected. There are very few balloons built that have a leakage less than 3%. The very best 'cæcum' balloons ever made have a leakage of ½% in 24 hours. The total buoyancy of the gas was 12,000 pounds. The 3 men would weigh 500 and the balloon probably not over 1,700 pounds. Very tight balloons have been made in this country that would weigh for the same size about 900 pounds. This would give 80 days' flotation. It is probable that the computation calls for even a heavier balloon and also for carrying the car all the way. It is customary, however, to prepare the car so that it can be used as ballast and at the last use the ring of the balloon.

It is a great pity that more experience was not gained in a long voyage before attempting the extremely hazardous voyage to the Pole. The fact that the balloon was beyond control at the very start is very significant. It is doubtful if any aëronant living can release safely a balloon of 170,000 cubic feet capacity in a twenty-five-mile wind. Those who were present at St. Louis on June 16, 1887, will remember the extreme difficulty experienced in sending off the World balloon 160,000 cubic feet in a twenty-mile wind.

If plans had been made to keep the balloon at 6,000 feet or so the success of the voyage would have been better assured. By using a small pilot balloon it would have been easy to send the overflow into the smaller balloon and, after the larger had leaked out enough, the gas in the smaller balloon could have been sent into the larger and the smaller used as ballast. At 6,000 feet the danger of rain and sleet freezing on the balloon would have been avoided and the currents which are far steadier and more rapid would have reduced the voyage by

*His. W. Die Anatomische Nomenclatur. *Archiv. für Anat. u. Physiol.*, Anat. Abt., Supplement-Band, 1895, p. 156.

one-half or even one-third. It is not generally known that this country holds the record for a long-distance voyage.

On July 1, 1859, La Mountain and three others sailed from St. Louis, Mo., to Henderson, N. Y., 870 miles in nineteen hours. Such constancy and velocity of air currents is seldom thought of and could have been found only, as in this case, at a height of 6,000 to 8,000 feet.

The balloon route would seem the ideal method of reaching the Pole and the French are already planning for such a voyage in 1898. Let them first bring their balloon to this country and make the Atlantic voyage in the track of steamers where the least untoward event will not be absolutely fatal. Also, by all means, let the balloon be placed at a height of at least 6,000 feet. It is just as easy to keep a balloon there as close to the earth. At Mt. Washington (6,300 feet) there have been frequent cases of west winds of 100 miles and over per hour for 36 hours, and this would mean less than 40 hours for the trip.

Of course, the great question is as to the occurrence of storm and high-area conditions at the Pole similar to those in more southerly latitudes, and the evidence from weather maps made near the Pole seems to show a great similarity. If so, any steady wind near the Pole would give a straight course to the Pole and the same wind would give a straight course back to civilization. It is a pity that with so much interest centered in this enterprise there should be so many fakes of carrier pigeons, etc., started. It is very rare, indeed, that a carrier has been known to fly 1,000 miles and then only after being taken over some part of the course half a dozen times. For a carrier to go 1,500 or 2,000 miles is an unheard-of feat.

H. A. Hazen.

August 21, 1897.

SCIENTIFIC LITERATURE.

Agriculture in Some of its Relations with Chemistry. By F. H. Storer. New York, Charles Scribner's Sons. 1897. Pp. 1901. $5.00.

The new edition of Storer's 'Agriculture' is in three volumes; it is accompanied by a very full and well-arranged index, which adds to its value as a reference work. The author is modest in his title, for not only is agriculture dealt with in its relations to chemistry, but very largely in its relations to the allied sciences, physics, physiology, botany and zoology.

For example, the contents of Volume I. include thirteen chapters, and in the first six the relations of physics and plant physiology to agriculture are discussed quite as fully as are the relations of chemistry in the remaining chapters. In Chapter III. 'Relations of Water to the Soil' is discussed, and in Chapter IV., 'Movement of Water in the Soil;" Chapter V. is devoted to a study of the principles of 'Tillage,' and Chapter VI., 'Implements and Operations of Tillage.' In these branches chemistry is not the primary science involved. In Chapter I. 'The General Influence of Soil and Air to the Plant' is treated, and in Chapter II., 'The Atmosphere as a Source of Plant Food.' In the discussion of these subjects the importance of the relations of plant physiology are fully recognized.

I note the above in order to show the general scope of the work; it is not limited to a setting forth of the relations of chemistry, as it could not well be and give a broad view of scientific agriculture.

That the author regards the question of manure to be of very great importance is shown by the elaboration of the various topics entered into, especially concerning the sources of supply, the modes of action and methods of using the various products. Seventeen chapters, seven in Volume I. and ten in Volume II., aggregating over 750 pages, are devoted to this one branch of scientific agriculture, and in which chemistry *is* the primary science involved.

His method of treating of the artificial fertilizers is worthy of particular mention. Not only is the theoretical consideration concerning the composition and character of the various fertilizing materials and their modes of action given, but numerous experiments from leading authorities are cited, thus putting before the student the original source of the information; a point often greatly desired by those who have not had a broad training in these lines, and

are thus not fully acquainted with the literature of the subjects involved.

Particular attention should also be called to Chapters XVIII., XXVI. and XXVII., concerning 'Humus or Vegetable Mould,' 'Lime and Lime Compounds,' and 'Sodium Compounds.'' It is eminently desirable that the information given in these chapters should be more generally disseminated, in view of the useless dissertations on these subjects by irresponsible writers. Humus, particularly, is an engaging topic for discussion by those who do not understand what it means. The same is true of lime—fruitless discussions are entered into, largely because of the lack of knowledge concerning the principles involved. So, too, with the matter of sodium as a fertilizer; papers teem with articles that are calculated to lead astray rather than to fix valuable truths in the mind of the farmer. Such articles also seem to possess a peculiar attraction for the general reader, and the result is 'confusion worse confounded.'

In Chapter XV. of Volume II. the subject of 'Symbiosis, or Blended Growth,' is discussed in the light of the recently acquired facts concerning this very important subject

That the legumes may and do, under proper conditions, use atmospheric nitrogen, is one of the most important recent discoveries in agricultural science, and the matter in its scientific and practical relations is fully and clearly set forth.

Volume III. contains fourteen chapters. It treats more particularly of 'The Theory and Practice and Systems of Crop Rotations,' the principles involved in and the advantages of 'Irrigation,' 'The Use of Sewage,' and 'The Growth and Management of Cereal Crops,' hay and pastures and the 'Making of Silage.' In the handling of these subjects, not only are the principles of chemistry as applied to agriculture well traced and made plain, but important practical suggestions are made concerning the economical principles involved in the general management of farms and in the growth of the various crops. The chapter on irrigation, while written from 'the point of view of a New Englander,' is, in view of the necessity of fully utilizing the natural advantages of the East, very timely, since it calls attention to the subject and points out conditions which are necessary for successful irrigation. Much of historical interest is included, also. The chapter on 'Ensilage'—it should be 'Silage'—is also up-to-date and includes much of value not otherwise readily accessible.

On the whole, the work may be regarded as of very great value to students and farmers alike, and should be in the library of every progressive man. The reader or student who takes up these volumes cannot fail to be impressed not only with the vastness of the subject included under the title 'Scientific Agriculture,' but also with the very great necessity to farmers of the present day of a knowledge of the principles upon which agriculture is based.

It may, perhaps, seem strange to the average person, that so little of the work of American investigators is cited by the author, for, with the exception of the Chapter on 'Silage,' comparatively few references are made to experiments conducted in this country. This is, doubtless, due in part to the fact that our experiment stations are comparatively new institutions, and that thus far but few workers are engaged upon purely scientific problems; much, however, of scientific interest, and that would add to the value of the work, has been overlooked.

While the work is a veritable storehouse of information, there is a profuseness of statement and an elaboration of details which seriously detracts from its usefulness, either as a ready work of reference or as a text-book for the student or progressive farmer. Sharp, clear-cut statements of principle and of fact are attractive, and appeal quite as strongly to the general reader in works of a scientific nature as in other lines of literature.

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The Sense of Beauty, being the Outlines of Æsthetic Theory. GEORGE SANTAYANA. New York, Charles Scribner's Sons. 1896.

So much has been written upon the theory of Æsthetics which, from the point of view of the trained intelligence, is nonsense pure and simple that the appearance of so noteworthy a

book on the subject as that of Dr. Santayana ought to receive special mention in the pages of SCIENCE. For the scientific man must ever have before him as a warning, the sad loss of the æsthetic and literary sensibilities which befell Darwin—a loss which, however much one may be given up to an absorbing pursuit, it is easily possible to prevent if one will but give oneself now and then a full and unrestrained bath, of short duration it may be, in some form of æsthetic enjoyment or of æsthetic speculation. If the college professor can spend his summers in painting pictures, as more than one college professor in this country is known to do, he will be reasonably sure to keep himself a happy human being and at the same time to detract nothing from his sum-total of scientific energy. But without so much of a draft upon time and native powers as this implies, a degeneration of the æsthetic faculty can be warded off in simpler ways—by a few months' absorption now and then in European picture galleries or in Swiss mountain scenery, by attaching to oneself a few artist friends if opportunity permit, by a breath of vigorous English poetry before going to sleep at night, or by many another similar device. While Darwin is the great example of a confessed total atrophy of the æsthetic feeling, it cannot be denied that in England the scientific man is more frequently a man of wider culture and experience, that scientific society is less likely to be unbearably monotonous and dull, than in this country. No doubt there is a larger number of cultivated families in England out of which the scientific contingent may be recruited, but at the same time that peculiar American energy which enables many a poor boy to become a master in his chosen field of intellectual activity would enable him at the same time to do something more, if he were once to be convinced that the charm of living, and hence the only pleasure of living (aside from the low pleasure of a gratified ambition), is indissolubly connected with the development of the æsthetic sensibilities.

The point to be particularly insisted upon in connection with the book before us is that *reading about art* is sometimes no less effective than the work of art itself, not only in turning the attention to such appeals to artistic enjoyment as may fall within one's field of view, but also in producing an actual sharpening of those senses through which the art appeal is made.*

The follower of scientific pursuits will do well, therefore, if he does not fail to make himself acquainted with so keen and luminous an imparting of the nature of the feeling of the artist in the presence of the work of art as this book contains.

In gratitude for so much that is of value, the dispassionate reader will doubtless be able to overlook, or at least not to lay up against the author, the passages of a silly and sickly sentimentality, in which he maintains, that a zealous philanthropy and a pure love of science are but the fires of stifled sexual passion bursting out in a different form. A thesis so wide of the mark as this will be as repugnant to the true artist as it is to the clear-brained psychologist, and it will find its audience only with those who have been made blind to the healthy aspects of human life by the novelist of degenerate France.

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* The present reviewer has a definite experience to communicate upon this point. Upon one occasion I had spent the whole afternoon in reading Charles Auchester (a marvellous book, when we consider that its author was only seventeen years old when it was written). I had been completely absorbed in the book, and had had no other thoughts for several hours. I then dressed quickly, and went out to dinner. The people at the dinner table were all well known to me and I was not expecting anything unusual, but I found to my surprise that I heard them with new ears. I perceived that their voices had a thousand shades of meaning, revealed a thousand qualities of character and mood, that I had hitherto been deaf to. It even seemed to me a kind of immodesty to perceive their bare souls so plainly as I now did through their voices. I had to use, in order to describe this experience, phrases similar to those which are common for vision—my ears were opened; I felt that hitherto I had heard as through a fog dimly; my ears had become unveiled; it seemed as if a lot of obstructing layers had been peeled off from my organ of hearing. If I could only have kept up this high tension of the aural intelligence and the aural sensitiveness I should, no doubt, have become easily a person of better capacity for the enjoyment of music that I am now, as well as a keener critic of my fellow men.